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FERAL MAN AND EXTREME CASES OF ISOLATION

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Feral man is the term for extreme cases of human isolation: either, as the present cases of the Wolf-children of Midnapore, of abandoned infants adopted and suckled by animals; or of older children who have wandered away into the wilds to survive by their own efforts, unaided by human contact.

The name and concept of feral man was introduced into organized science in 1758 by the great classifier, Linnaeus, in the tenth and subsequent editions of his *Systema naturae*.¹ His account was based on the poorly attested cases reported in the historical sources prior to his time. These cases are the following.

- (1) The Lithuanian Bear-boy, 1661.
- (2) The Hessian Wolf-boy, 1344 (Linnaeus mistakenly dates this as 1544).
- (3) Irish Sheep-boy reported by Tulp, 1672.
- (3a) Bamberger Cattle-boy, end of the 16th century (a poor case).
- (4) Wild Peter of Hannover, 1724.
- (5) The Pyrenees boys, 1719 (a most dubious case).
- (6) The girl of Cranenburg, 1717 (*Puella trans-isalana*, Linn.).
- (7) The Songi girl from Champagne, 1731 (*Puella campanica*, Linn.).
- (8) Jean of Liège.

As will be indicated in the tabular summary (pp. 500-503), Linnaeus, from the inadequate data, could have concluded little more than his terse characterization of feral man as *mutus*, *tetrapus*, and *hirsutus*. The last, *hirsutus*, is certainly in error since few of the cases are reported as hairy.

Buffon introduced the data and concept of feral man to the French about 1750 in his famous works. Apparently from him the concept fell into

* Accepted for publication May 5, 1940.

¹ Carl von Linné, *Systema naturae*, 10th ed., I, 1758, 21.

the unscientific hands of Rousseau, who, in 1754, in his famous essay on inequality² appears to have found the case of the Hessian Wolf-boy as congenial to his simplistic philosophy of environmental determinism. This radical thesis down to the present day has done more than anything else to prejudice the case of feral man.

It was thirty years after the brief romantic treatment of feral man by Rousseau before the next important literary and philosophic discussion of feral man. The German romantic philosophy treats feral man in Herder's great work, *Ideen zur Philosophie der Geschichte der Menschheit*,³ still cited in histories of anthropology. Herder's citation of feral man has no such sweeping theoretical implications as that of Rousseau. Herder's discussion is merely in connection with his treatment of the upright position of man, from which Herder mistakenly uses feral data in a preëvolutionary argument against man's descent from lower forms of animals.

Herder apparently became acquainted with the data of feral man through Schreber's contributions to German knowledge in an encyclopedic natural history similar to Buffon's earlier and more famous works. In his discussion, Schreber gives the detailed data on eight of Linnaeus' nine cases of feral man, putting (3a), the Bamberger case, in a foot-note, where it deserves to be. To the remainder of Linnaeus' cases, Schreber adds the following.⁴

(9) The second Wolf-boy from Germany, that of Wetterau, 1344.

(10) The second of the Lithuanian Bear-boys, that of 1694.

(11) Later the data on a third Lithuanian Bear-boy were added to the literature from another source of 1698;⁵ comparable to the bear boy of India reported alive in 1893 as cited by Sir James Frazer (see p. 491). This case is not included in the tabular summary because no data are given other than failure to master the upright position and language.

In France scientists were opposing the romantic and revolutionary thesis of Rousseau as late as 1812. In that year, Gall and Spurzheim, the fathers of phrenology, generalized on the subject of feral man unfavorably, without much regard for the facts of the particular cases except for brief notes

² J. J. Rousseau, *Discours sur l'origine et les fondements de l'inégalité parmi les hommes*, 1754.

³ J. G. Herder, *Ideen zur Philosophie der Geschichte der Menschheit*, *National Litteratur*, 77, 1784, 1070-1109.

⁴ J. C. D. von Schreber, *Die Säugethiere in abbildungen nach der Natur mit Beschreibungen* (8 vols.), I, 1774-1846, 31.

⁵ The original account of the third Lithuanian Bear-boy is given by the English physician to the Polish King, Bernard Connor, who saw the second case and quotes a letter from a witness of the third, the Netherlands Ambassador to London, J. P. van Kleverskerk. See Bernard Connor, *The History of Poland*, I, 1698, 348-350.

on two other possible cases not mentioned elsewhere. They are, however, too inconsequential to be included in the tabular summary.⁶

More substantial scientific opposition to Rousseau's romantic philosophy of feral man was implemented by the not altogether fair realism of Blumenbach, a great German scientist, whom the physical anthropologists acknowledge as the father of their discipline. In 1811 Blumenbach unfavorably reviewed the whole problem of feral man, giving the data in some detail, even if inadequately and unfairly, on all cases of feral man cited by Linnaeus.⁷

Wild Peter was caught naked in a field in Hannover in 1724 and showed the characteristics of a feral case. Like many other such, he reached the hands of the ruler, in this case King George I of England and Hannover. Partially on this account Wild Peter attracted much attention and considerable material on him has come down to us. Almost a century later Blumenbach not only went through the English data but also through the Hannoverian archives in Hameln and elsewhere. This makes his paper an excellent source on Wild Peter even though we do not agree that Wild Peter has no more interest than an ordinary idiot. Though he lived to be 80 years of age and was not deaf he never learned to speak, easily wandered off and got lost, and could never be trusted with even the simplest task unsupervised.

When Wild Peter was first found he hated confinement, cunningly succeeded in escaping, and was filthy in his habits. At first he did not like bread but would peel green sticks to chew the juice out of the bark, besides eating cabbage, grass, and bean-plants. According to Blumenbach's critical data, Wild Peter supported himself for a year in the woods where he had been driven by his father, one Krüger, to whom he returned after a year, quite tattered. Again he was driven away with blows, like a strange dog, by his new step-mother.

One wonders if a complete idiot could have survived in the wilds, a year at a time unseen by human beings, until at least a year later, by sailors passing down the river, just before he was captured. Nor do Wild Peter's pictures, which Blumenbach says he was assured in England presented a good likeness, show Wild Peter as a hopeless idiot, but rather "a right good-looking old man, whom you would consider quite bright, if we didn't know better."

⁶ F. J. Gall, and G. Spurzheim, *Anatomie et Physiologie du system nerveau en general et du cerveau en particulier*, 1812, 41-42.

⁷ J. F. Blumenbach, *Von Homo sapiens feras Linn. und namentlich von Hammelschen wilden Peter*, *Beiträge zur Naturgeschichte*, 2nd pt, 1811, 10-44. This paper has been translated into English by Bendysche, *The Anthropological Treatises of Johann Friedrich Blumenbach*, 1865, 1-35.

Whatever Wild Peter's mentality was, imbecile and tongue-tied as he is represented to us, we cannot today think but that the tragic abuse that he had received from an unnatural parent and from his step-mother must have been some factor in his retardation. Whether or not this would now be admitted by Blumenbach, he did science a service in critically examining the new data he gathered, as well as in blasting the romanticism of Rousseau's "natural man." Still Blumenbach may be criticised for unfair treatment of some of Linnaeus' other cases, and for omitting the more important cases which had attracted wide attention in the interval. Such a case was that of the Wild Boy of Aveyron of 1799, who was observed by competent and distinguished scientists in France.

Blumenbach's critical review did not succeed in banishing feral man to the limbo of mythology. In 1830 Rudolphi, one of Blumenbach's first students of anthropology, as it was then understood to include physiology, was impelled to discuss the problem.⁸ Blumenbach's argument against feral man was not, however, to be adequately answered until 1848. In this year appeared Tafel's substantial contribution to the problem of feral man. In his *Fundamentalphilosophie*,⁹ he collected all the data on most of the cases in Europe of feral man. The work itself is a significant treatment of pre-Darwinian evolutionary thought; and, though it has never been cited in English histories of science, it well might be. Instead of dismissing and slurring the data other than for Wild Peter, as Blumenbach had done with the other cases, Tafel gives us all the data on Linnaeus' cases and adds several others.

After presenting the data in the manner closely followed by all the later writers who have presented them, Tafel then argues, in direct opposition to Blumenbach, that none of these cases involves congenital idiocy, citing writers on idiocy current in his day. He argues that the condition of feral cases is the result of their isolation from human society.

Besides the European cases prior to 1848 reported by Tafel, the horizon of feral man was greatly widened and its data enriched by reports in 1852 of six cases of wolf-children from India, similar to the recent ones of Midnapore. The distinguished author of the former account was Major General Sir William H. Sleeman, K.C.B.,¹⁰ who spent 1849-50 in the Indian

⁸ K. A. Rudolphi, *Grundriss der Physiologie* (2 vols.), I, 1830, 25-29.

⁹ J. F. I. Tafel, *Die Fundamentalphilosophie, in genetischer Entwicklung mit besonderer Rücksicht auf die Geschichte jedes einzelnen Problems*, 1st pt., 1848, 44 ff.

¹⁰ *An Account of Wolves Nurturing Children in Their Dens*, by an Indian Official. Jenkin Thomas, Printer, 9 Cornwall Street, Plymouth, 1852. Apparently unaware that the identical account had appeared in Sleeman's book (*A Journey Through the Kingdom of Oude*, London, Richard Bentley, 1848, 2 vols.); the material on the wolf-children was reprinted in the organ of the Linnaean Society, *The Zoologist*, 3rd ser., 12, 1888, (no. 135), 87-98.

Kingdom of Oudh (N. W. India), the region in which Midnapore is located. Here he was engaged in the suppression of the Thugs. General Sleeman recounts two cases which he appears to have seen personally, and four others of which he heard through persons on whose integrity this competent observer was willing to risk his high reputation for veracity. There is another case, that of an old man whom he himself saw, who may or may not have been reared among wolves. This case, unlike the others, is not included in the tabular summary.

Besides Sleeman's six cases of feral man, which brings the total to 17, several later cases from India have been sufficiently adequately reported and agree so well with the recent Midnapore case as to be worth consideration and inclusion in the tabular summary. These cases are:

(18) Case reported from Sultanpur;¹¹

(19) Wolf-boy of Shahjejanpur;¹²

(20) Dina Sanichar, the Wolf-child of 1867 at the Sikandra Orphanage;¹³

(21) Second Sikandra Wolf-child of 1872.

Numerous other reports of cases from India are unsatisfactory.

The publication of General Sleeman's account of the wolf-children from India attracted to the problem of feral man, the great English cultural anthropologist, E. B. Tylor, who is rightly called the father of that subject in England. This new material, Tylor says, "makes it necessary to re-argue the question whether children have ever been carried away and brought up by wild animals,"¹⁴ which had been dismissed by Blumenbach, the father of physical anthropology. Though Tylor agrees that, on his inadequate data, Blumenbach could not have done otherwise, Tylor's contribution shows the sharp divergence between physical and cultural anthropology which has carried down to the present in the problem of feral man. Tylor's acceptance was followed by Galton,¹⁵ and Sir James Frazer¹⁶ who cites

¹¹ H. G. Ross, *The Field* (London), 9, 1895, (no. 2237), 786.

¹² Anon., Wolf-children *Lippincott's Magazine*, 61, 1898, 121.

¹³ Valentine Ball, *Jungle Life in India*, London, de la Rue and Co., Bunhill Row, 1880. For other materials see, India's wolf-children found in caves, *Literary Digest*, 95, 1927 (Oct. 8), 54-56. A copy of Dina Sanichar's obituary and his picture as well as a copy of the record of his admission were sent me (1939) from the Orphanage. The wolves from which he had been rescued were brought dead with the child to an English magistrate for the wolf-bounty. The name of this magistrate is elsewhere given as Mr. Lowe, a son-in-law of Sir W. Muir; but no official report appears in the literature, much less from the skeptical correspondent, W. F. Prideaux, *Wolf boys, Notes and Queries*, (6th series), 12, 1885, 178.

¹⁴ E. B. Tylor, Wild men and beast-children, *Anthropological Review* (London), 1, 1863, 21-32.

¹⁵ Francis Galton, The domestication of animals, *Transactions of the Ethnological Society of London*, N.S., 3, 1865, 136.

¹⁶ James Frazer, *Fasts of Ovid*, II, 1929, 269-281.

a bear-boy in India reported alive in 1893, reminiscent of Lithuanian bear-boys of 1698.

Tylor's paper cites a report of missionaries from Polynesia of isolated native survivors of a massacre which is not strictly a feral case and so is not included in our tabular summary. Similarly not included is the case, that Tylor also accepts as probable, recorded from ancient Rome at the time of the conquest of the Goths. It is strange that myth should account for the founding of Rome by Romulus and Remus, and that history should record at its fall a similar case of a child suckled by a goat among the ruins of one of its villages.

The chief contribution of Tylor's paper to the data on feral man is the record of two cases from Overdyke, an asylum for foundlings established in Germany after the horrors of the Napoleonic Wars, when many children were lost or abandoned in the forests and fields. The second of the Overdyke boys had apparently lived almost wild in the forest. He climbed trees with remarkable ability to get eggs and birds from the nests. The eggs and birds he devoured raw, a habit of which he was never cured.

The other Overdyke case reported by Tylor was Clemens, a child who had been set out to herd pigs and given so little food that he had to suck milch sows and eat herbage with the pigs. When he first came they had to keep him out of the salad beds as though he were a pig himself. The Overdyke cases are (22) and (23) in the tabular summary.

Number (24) is another case of a child who had lived with swine. She was seen in Salzburg and reported by Horn.¹⁷ She was 22 years old, and by no means ugly despite having been brought up in a hog-sty among the hogs. She had sat there for so many years with her legs crossed, that one of them was crooked. She grunted like a hog and her gestures were brutishly unseemly in a human guise.

We must turn from the sole general discussion of feral man in English to the German for the next consequential contribution, that of Ráuber of 1888.¹⁸ Rauber, apparently unfamiliar with Tafel's earlier and even better complication of the materials, again presents the data on the classic cases of feral man. To these he adds several, one of which, that of the Wild-boy of Kronstadt, is well worth inclusion in our tabular analysis as (25). Rauber's general grasp of the psychological implications of the problem of feral man is distinctly modern. Rauber's conclusions may be summarized in this quotation:

¹⁷ Wilhelm Horn, *Reise durch Deutschland, Ungarn, Holland, Italien, Frankreich, Grossbritannien, und Ireland* (2 vols.), 1, 1831, 138.

¹⁸ August Rauber, *Homo Sapiens Ferus oder die Zustände der Verwilderten und ihre Bedeutung für Wissenschaft Politik, und Schule*, 2nd. ed., 1888.

"We must not forget that the human child learns more during the first two years of his life under the influence of his living surroundings than in all the entire period afterwards. . . . If these influences fail, let us say until the fourth, sixth, or tenth year, how could it be possible that this could be without influence in the normal development of the brain?"

These cases of feral man group themselves into two classes: (a) those who have wandered away into the wilds to survive by their own efforts unaided by human contact; and (b) children nurtured by wild or domesticated animals. Strictly not feral cases are the similar ones of children shut away from human association by cruel, criminal, or insane parents. These furnish the commonest case and are reported in the press from time to time. They show the same effects from isolation; but they more commonly recover normal minds and personalities not only due to the shorter period of isolation involved before they attract the attention of neighbors or child protective societies, but also due to the fact that they do not have animal or other wild conditioning to unlearn. To this group belongs the famous case of Kaspar Hauser. His case, like others of this sort, is rightly classified by Tredgold as *Amentia* of Isolation,¹⁹ as also are the deprivations suffered by the deaf and dumb.

(a) True feral cases are rarer than *isolation amentia* in these times of modern communication and contact. Of feral cases the rarest are those of children lost in the forests to survive to adolescence. Yet in the disasters of the great wars of the 18th and 19th centuries several cases were reported on the scientific authority of men like the great English anthropologist, Tylor (Second Overdyke case), or an educator like Itard (The Wild Boy of Aveyron). We may suspect that in post-war Russia, among the hordes of abandoned children, some may have wandered away into the wastes; and, but for the rigors of the climate, may have survived. In the present wars in China and Spain, children may have been reduced to this feral condition. As the publication of each new case in the past has brought reports of others, perhaps this publication may bring others to light.

The Wild Boy of Aveyron,²⁰ who appeared in 1799, offers our best evidence for a case of this type. During that year, in France, hunters pulled a boy out of a tree, who had apparently got lost or wandered off into the woods as a child and survived until found at 12 years of age as a wild creature living on nuts, and berries. He gave no evidence of nurture by animals and could walk, run, and climb like a wild thing. He was examined by Pinel, the leading psychologist in France of that day. Pinel called him an incurable idiot, despite the fact that he had had animal intelligence enough to have lived alone in the wilds from nature's products, unaided by man or animals. Not daunted by the pronouncement of the great psychologist, the education of this wild boy was undertaken by Itard, a doctor of medicine and a teacher in a special school for the deaf and dumb. Itard gives a full account of the education of this wild boy. By this we see that with the best of educational method for

¹⁹ A. F. Tredgold, *Mental Deficiency (Amentia)*, 1920, 304 f.

²⁰ J. M. G. Itard, *The Wild Boy of Aveyron*, Translated by George and Murial Humphrey, 1932.

deaf, dumb, and sub-normal children (which was surprisingly well-advanced in France by the educational theories of Rousseau, set in motion by the French Revolution then at its height), this wild boy lost his bestial appearance and manners and became an appealing but much sub-normal person. He never learned more than a few words before his death at 40 years of age. Kamala, the elder of the Wolf-children of Midnapore, learned more words, about 50, and could use some of them intelligibly in short sentences before she died at seventeen after 9 years of education. The Wild Boy of Aveyron is analyzed here in the table as case (26).

Other cases of feral man of this type made better recoveries, much greater than those recorded for feral children reared by animals, as Kamala was. Cases that recover are of the greatest significance for this study. In science it is the exception that proves the rule. Thus of particular importance are the cases of regaining the ability to speak fluently together with the other attributes of a developed human personality.

Such a case is (7) in the tabular analysis, the Wild Girl of Songi (Champagne), whom Linnaeus named the *Puella campanica*. The sources on the *Puella campanica* are inadequate and especially incommensurate with the importance of this case. They are incomplete or defective, especially with regard to how long she had lived in isolation and regarding the mysterious lady who had first undertaken her education, taught her to embroider, etc., before she appeared at Songi. Also the primary data are marred by fantastic theories of her origin as an Esquimo or on the Island of "Guoadalupe." Still we are inclined to admit Rauber's conclusions "that it is impossible to doubt either the intelligence of the girl, or her former wildness."

The defects in the data are the more lamentable because of the crucial light that the case of this girl of Songi could throw upon the question of isolation and feral man, due to her not inconsiderable recovery of human faculties. The record would seem to merit some consideration since it cites contemporary persons of importance, such as the Intendant of Champagne and the Bishop of Chalons, the latter the one who provided her with asylum in a Catholic hospital.

The girl of Songi is the only case of this sort, so far recorded, who recovered speech, to talk distinctly if somewhat brusquely. She clearly seemed to be intelligent since the record shows that she learned easily what was shown her. That the growth of her intelligence was sufficient for her to assimilate enough of the Catholic religion to become a nun seems certain enough. But, through the defect in the records as to the degree of her exposure to isolation, no conclusions seem justified despite the remarkable recovery of the frog-and-fish girl recorded in the beginning to become a devout nun, who had assimilated enough of the ability to speak and of the Catholic religion to say:

"Why should God have searched for me and saved me from the power of wild animals, and made me a Christian? Should this have happened in order to leave me and make me die from hunger? This is not possible. I know only Him; He is my Father and the Virgin Mary is my mother; they will take care of me."

In early anthropological sources there are records for another interesting case of substantial recovery from a half-wild life caused by human neglect of a fellow creature. The early writer on anthropology, Michael Wagner,²¹ gives data on this interesting case, not cited by any of the previous writers, apparently because it is

²¹ Michael Wagner, *Beyträge zur Philosophischen Anthropologie und den damit verwandten Wissenschaften* (2 vols.), I, 1794, 259-264. (Copy in Harvard Library.)

not a case of completely feral man. Wagner extracts from the records of Zips, Hungary, the account of a poor creature who, through human neglect, had fallen into a half-wild condition. That he had lived in some degree of human association was shown by his gratitude for favors. Under the decent care of the unknown scribe, whom Wagner quotes, his mental condition improved rapidly, though his physical condition presaged an early death. He learned to speak Slovak sufficiently to be largely intelligible to his benefactor; and, though he persisted in an invented language of his own, he came to understand German. His case offers comparisons to that of Kaspar Hauser in the improvement noted, as well as in the convulsive movements that he fell into under emotional strain. Like all other cases, this man from Zips showed a surprising indifference to sexual impulses, though he was a mature man. This case is (27) in the tabular summary.

The most significant case of recovery, and one with the best attested data, is Kasper Hauser (28). This is a case of *isolation-amentia*; but it is especially comparable to feral ones because his only remembered associations were with toy hobby-horses. These conditioned him to horses as feral cases are conditioned to the species of their animal associates. From the lack of real animal association, he had, however, nothing to unlearn.

Kaspar Hauser appeared in Nürnberg in 1828 at about 17 years of age. He was barely able to walk a few steps, but could write the name Kaspar Hauser by which he has always been known. He was unable to utter more than an unintelligible parrot-like phrase, "A Reutä Wähn, wig mei; Vötta Wähn is," perhaps very garbled German for "I wish to be a rider as my father is." He also repeated the word *Ross* (Bavarian dialect for "horse"). When placed in a jail cell, he was given a child's hobby-horse which he played with for hours on end, oblivious to everything else. He could eat only bread and drink only water, on which he later said he had lived as long as he could remember. When he appeared he was so weak that he could scarcely lift the hobby-horse across the threshold.

He became the ward of the town of Nürnberg and was placed under a very competent teacher, G. F. Daumer, who has left us an excellent and scientific account. He also attracted the attention and guardianship of one of the greatest of the jurists of Bavaria, Paul J. Anselm, Ritter von Feuerbach, whose authority is again unimpeachable for honesty and veracity.

Kaspar Hauser, since childhood apparently, had been shut up in a cellar dungeon and fed bread and water as long as he could remember later. That he was fed and cared for physically shows some evidence of human contact and association, which probably had something to do with his remarkable recovery of human qualities. In his dungeon cell he had had two hobby-horses and a toy dog, which accounts for his crying "Ross" and playing with a child's hobby-horses for hours at a time when he was released and so unexpectably appeared before the town gates of Nürnberg.

From lack of all other association and no childhood association with animals, Kaspar Hauser appeared into this world as a *tabula rasa* rather than as animal-conditioned. In the five short years of life accorded to Kaspar Hauser, he lived a whole life, developing mentally from a child of three years in an adolescent body of seventeen who played with hobby-horses, to a young man who spoke and wrote fluently and served Feuerbach as a legal clerk for a short time.

Due to deformations in his legs from having sat all his youthful years in his narrow cell with his legs stretched out before him, he never learned to walk well

or to run. But due to his years of playing with hobby-horses, he loved real horses and had no fear for them. His weak legs, calloused from life-long sitting, were perfectly adapted for riding and he immediately developed into a horseman who could out-ride the troops. He had apparently been given hobby-horses to create in him this interest with the intention of his jailer that he should join the Bavarian cavalry. When he appeared in Nürnberg he carried a letter to the cavalry commander, asking him to be put in this service.

Instead, however, of getting lost in the army, he became the famous ward of the town, known all over Europe who attracted visitors from everywhere. When it was broadcast that he had developed so much that he was able to write his autobiography, the same forces that had ordained this "crime against a human soul" of his incarceration, climaxed it by assassinating this harmless and attractive youth who had won the pity and admiration of Europe.

Who was Kaspar Hauser? Why was he kidnapped, imprisoned, and finally assassinated? All puzzles, now insoluble, that have been argued pro and con in over a thousand books, pamphlets, and articles.²² Now that the German petty kingdoms are no more, it is safer to investigate than when Feuerbach was stopped in his search by the "colossus that guards the gates of certain castles;" but it is too late to question witnesses. Modern opinion in Germany agrees with the hints of Daumer and Feuerbach that Kaspar Hauser was the legitimate heir to the throne of the petty Grand Duchy of Baden,²³ who had been sequestered soon after birth to make way for themorganatic Hochberg line, but who was kept alive as a hostage lest the plot go awry.

The historical aspects of this great unsolved crime of a century ago are as interesting as a romance, but the scientific interest is of far greater value and rests on better evidence. This is one of the rare cases of isolated individuals to regain almost a complete human personality. The best literature on Kaspar Hauser, his own autobiography, and the evidence of those who knew him best has lately been collected in Germany.²⁴

(b) The strangest form of feral condition is that of human children suckled by animals. Such a case is the recent one of the Wolf-children of Midnapore, which is the best attested on record because several persons are recorded as actually seeing the children living as animals among animals. The almost unique element in the recent case is not the adoption of infants of wild animals; but the fact that only in the present case do we have a careful record of nine years in human association after the rescue by the man who, in company with several others, saw the children in association with the animals which they killed before the children were brought again into human society.²⁵ The Midnapore girls, Amala and Kamala, are tabulated

²² Peitser-Ley, *Kaspar Hauser*, Druck von C. Brugel & Sohn AG, Ansbach.

²³ The best account in English of the Hauser-legend is by Elizabeth Edson (Gibson) Evans, *The Story of Kaspar Hauser from Authentic Records*, 1892. As late as 1930 the same hypothesis was re-stated in German by Luise Bartnig, *Altes und Neues zur Kaspar Hauser*, 1930.

²⁴ Herman Pies, *Kaspar Hauser*, 1926. Here included is Feuerbach's important account which was translated into English more than a century ago, *Kaspar Hauser* translated by H. G. Linberg, London, 1833, Simpkin and Marshall.

²⁵ J. A. D. Singh, The wolf-children of Midnapore, (manuscript unpublished). See P. C. Squires, "Wolf children of India, this JOURNAL, 38, 1927, 313-315; W. N. Kellogg, More about the "wolf children" of India, *ibid.*, 43, 1931, 508-509; A further note on the "wolf children" of India, *ibid.*, 46, 1934, 149-150.

as numbers (29) and (30). In all other such cases, the children pass through several hands after the rescue from animals, before reaching the person who recorded the case.

The entire status of the problem of feral man has hitherto been uncertain because of the lack of recorded witnesses of the animals as foster-mothers to human beings. Cases reported as secured from wild or domesticated animals have just been reviewed from the historical records of Europe from the 14th century (the Hessian Wolf-boy) down to the nineteenth (Clemens of Overdyke). From India we have seen that reports of this sort, as the present cases, have been much more frequent. This is probably because wolves are not killed on superstitious grounds and steal many children every year. Apparently a mother-wolf in suck occasionally adopts one of the victims. In the region of India surrounding Midnapore, children reported as rescued from wolf foster-mothers have been so frequent since the English entered the Northwest Provinces (Bengal) in 1850, that it actually appears that one or more wolf-children have been under institutional care somewhere on that continent, ever since that time. Though Kamala died in 1929 there are reputable accounts of others that are worth checking, who are possibly alive in India today.

J. H. Hutton, William Wyse Professor of Social Anthropology, St. Catherine's College, Cambridge University, in the *London Times* of July 24, 1939, communicates the following reference to such a case, with which he became familiar while serving in the high scientific position as Director of the Census of India. This case reported in the *Journal of the Bombay Natural History Society* by E. C. Stuart Baker in 1920 is the more interesting because it is the only one involving a leopard foster-mother. There was an isolated and brief mention of this case,²⁶ which I took as fantastic, especially since so fierce an animal as the leopard was involved. Yet this case has been again brought to attention through so distinguished a correspondent to the *London Times* as Professor Hutton, whose experience in India makes him an excellent judge of its credibility. Such things counter-indicate rejecting this or any other case without every possible checking. The data on this case follow.²⁷

Before leaving the subject of feline senses it may be of interest to relate a story of a leopard child which has not yet ever been published though it was pretty well known at the time.

In the North Cachar Hills, where the boy was found . . . When questioning a man why he wanted exemption from such labor he told me that he had a little "wild" son to look after and as his wife had recently died, he could not leave the village to work or the boy would run back to the jungle.

I accordingly went outside the court to see the "wild child" and satisfied myself as to the truth of the story. There sure enough outside was a small boy about seven years of age, or less, squatted on the ground like a small animal; directly I came near him he put his head in the air and sniffed about, finishing by bolting on all fours to his father between whose legs he looked like a small wild beast retreating into a burrow. Looking closer at the child I saw that he was nearly or entirely blind from some form of cataract and his body was covered with the white scars

²⁶ Wolf children of India, *Living Age*, 332, 1927, 1020-1022. This article, like that in the *Literary Digest*, 95, Oct. 8, 1927, 54-56, is principally about a then current case, that of Maiwana, some 75 miles from Allahabad in British India. In 1927 he was reported in a mental hospital in Bareilly, but my correspondence with India on this case, has so far revealed only that the Institution is now closed, so I have added nothing to the meager notes given in the above articles, which are certainly worth checking farther.

²⁷ E. C. Stuart Baker, The power of scent in wild animals, *J. Bombay Natural History Society*, 27, 1920, 117-118.

of innumerable healed tiny cuts and scratches. Struck with his appearance I asked the father to tell me about the boy and he then narrated the following wonderful story which I fully believe to be true, but which my readers must accept or not as they think fit.

It appears that about five years before I saw the father and son, the Cachari villagers of a village called Dihungi, had found two leopard cubs close to their village which they killed. The mother leopard had tracked the murderers of her children back to the village and had haunted the outskirts for two days. The third day a woman cutting rice in some cultivation close to the village laid her baby boy down on a cloth whilst she went on with her work. Presently hearing a cry, she turned around and saw a leopard bounding away and carrying the child with it. The whole village at once turned out and hunted for the leopard and baby but without success and finally they were forced by darkness to leave the boy, as they supposed to be eaten by the leopard.

Some three years after this event a leopardess was killed close to the village by a sportsman who brought in the news of his success together with the information that the leopard had cubs which he failed to secure. On hearing this the whole village turned out and eventually captured two cubs and one child, the boy of this story. He was at once identified by his parents, claimed by them, and their claim admitted by the whole village.

Subsequently when visiting Dihungi I interviewed the head man and also the man who actually caught the child and they both corroborated the father's tale in every detail. It appeared that at the time he was caught the child ran on all fours almost as fast as an adult man could run, whilst in dodging in and out of bushes and other obstacles he was much cleverer and quicker.²⁸ At that time he was only suffering from cataract to a slight extent and could see fairly well, but after he was caught his eyes rapidly became worse. His knees, even when I saw him and he had learnt to move about upright to a great extent had hard callosities on them and his toes were retained upright almost at right angles to his instep.²⁹ The palms of his hands and pads of toes and thumbs were also covered with very tough horny skin. When first caught he bit and fought with everyone who came within reach of him and, although even then affected in his eyes, any wretched village fowl which came within his reach was seized, torn to pieces and eaten with extraordinary rapidity.³⁰

When brought before me he had been more or less tamed, walked upright when startled into extra rapid motion, was friendly with his own villagers whom he seemed to know by scent,³¹ would eat rice, vegetables, etc., and consented to sleep in his father's hut at night. Clothes, being a Cachari child of tender years, he had not been introduced to.

His blindness was not in any way due to his treatment by the leopard—if the story is true—as I found that another child, a couple of years older, and the other also had the same cataract. At the same time the defective sense of sight may well have intensified his smell as the loss of the one must have caused him to rely more on the other. When caught the child was in perfect condition, thin but well covered, and with a quite exceptional development of muscle.

The inadequate data in this case are tabulated as number (31).

There are other stories of feral cases in India that are still alive and should certainly be worth checking. Professor Hutton has also referred the writer to mention of such a case in the *Illustrated Weekly of India* (Feb. 5, 1933, p. 37) which gives the following meager details, which are now being checked by correspondence:

A British Officer at Jhansi has rescued a child from a pack of wolves which brought it up. This strange creature has been exhibited at the Gwalior Baby Week,

²⁸ Similar details given of the Wolf-children of Midnapore.

²⁹ The same feature is reported by Rev. Singh of the Wolf-children of Midnapore.

³⁰ Similar things are told of several feral cases.

³¹ Feral man is often reported as with highly developed sense of smell, though here it is probable that it was even greater due to compensation for defective sight.

presumably as a bait to see the more instructional of these excellent shows. At the moment Dr. Antia, the Chief Medical Officer of Gwalior, is trying to cure the child of the wild habits it has learnt from its wild companions in the jungle, and the method of progression on all fours has already been changed to the upright carriage. The Indian *beriga*, or wolf, is found in Rajputana particularly and in most other parts of India except Lower Bengal. Considering that it frequently carries off children besides sheep and goats, it is remarkable this child was allowed to live.

Another report of an Indian wolf-child who may still be alive is the Maiwana case of 1927.²² Though I have not been able to see the original reports from India, details on this case are given in *The Literary Digest*,²³ and the *Living Age*.²⁴ These reports would appear possible to check and well worth the trouble.

The value of careful checking in the field of the data of alleged cases of feral man is shown by the case of Lucas the alleged Baboon-boy of South Africa. The newspaper accounts of this case looked worth sending to Dr. R. R. Gates, of the University of Bristol, whose interest in this problem and whose strategic situation in London, enabled him to put almost all the facilities of the British Empire, including the well-known Letters column of the *London Times*, to the service of this study. Dr. Gates forwarded the clippings to Dr. Raymond Dart, Dean of the Medical School of the University of the Witwatersrand. Dr. Dart's importance in South Africa is sufficiently great to have caused the highest officials in South African Mounted Police to check their records back to 1904, where there was convincing but hear-say evidence from a constable that Lucas had been rescued from baboons by two constables, now dead. It seemed sufficient basis for the recent preliminary notes on this case that were published in both the popular and scientific press.²⁵ Continuing careful work in South Africa through the researches of Dr. Dru Drury and Dr. van Heerden, however, broke down completely any claims for Lucas as a case of feral man.²⁶

The present writer has unfortunately been unable to get in touch with scientists in India to check and re-check similarly the cases reported from there. Through the good offices of men familiar with India through much travel or long residence, such as Professor R. R. Gates and Professor J. H. Hutton, every effort, however, has been made to check the current cases. The report of Rev. J. A. L. Singh is attested to not only by Bishop H. Pakenham Walsh, Tadagam P. O. Coimbatore, but also was investigated by the English magistrate of Midnapore, who went over Rev. Singh's material on the spot. Publication of the valuable document, Rev. Singh's diary account of some 150 pages of the rescue and life in human society of the two wolf-children of Midnapore, has been held up for almost three years by me while working with many other interested scientists making every effort to check the account. This labor has not succeeded in breaking down any of the data reported by Rev. Singh.

Not so much for this case but for the others apparently still alive in India, it might be worth while for one of the research institutions of the United States even to send scientists to gather and check materials on the spot.

²² The original sources on these cases are given me by Professor Hutton as the Indian periodicals, *The Englishman*, April 7, 1927; and *The Pioneer*, April 5, 1927.

²³ *Op. cit.*, 95, 1927, (Oct. 8), 54.

²⁴ *Op. cit.*, 332, 1927, (no. 4307), 1021 f.

²⁵ J. P. Foley, The "baboon boy" of South Africa, this JOURNAL, 53, 1940, 128-133.

²⁶ R. M. Zingg, More about the "baboon boy" of South Africa, this JOURNAL, 53, 1940, 455-462.

TABULAR SUMMARY FERAL MAN AND CASES OF EXTREME ISOLATION

Case	Sex	Years old when rescued	Years old when isolated	Obvious idiot	Ame- ate animal	Fierce behavior when caught	Animals unafraid of	Shares food with	Grew up wild in the forest	Hirsute (Hairy)	Terrapin, four-footed motion	Taught to walk	Muzz (dumb)	Made animal-like sounds
(1) First Lithuanian Bear-boy of 1661? (1657)	Male	13	3		Bear		X				X	X	X	
(2) Irish Wolf-boy, 1344	Male	7 to 13			Wolf						X	X		
(3) Irish Sheep-boy, 16	Male	16			Sheep	Escaped					X	X		Bleat
(3a) Bamberg Cattle-boy	Male		11?		Cattle				X		X	X	X	
(4) Wild Peter of Hannover 1724	Male	13						X			X	X	X	
(5) The Pyreness Boys 1719	Males	18						X	X		X	X	X	
(6) Girl of Cramenburg 1717	Female							X	X		X	X	X	
(7) Girl of Song, 1731	Female	21	5						X		X	X	X	
(8) Jean of Leige	Male				Wolf						X	X	X	
(9) Second Lithuanian Bear-boy, 1697	Male	10			Bear						X	X	X	
(10) Third Lithuanian Bear-boy, 1694	Male	12			Bear						X	X	X	
(11) Steersman's first Wolf-child	Male				Wolf			Dog			X	X	X	
(12) Steersman's second Wolf-child	Male				Wolf	Wild					No data	No data	No data	
(13) Steersman's third Wolf-child	Male				Wolf	X					No data	No data	No data	
(14) Steersman's fourth Wolf-child	Male				Wolf	No data					X	X	X	
(15) Steersman's fifth Wolf-child	Male				Wolf						X	X	X	
(16) Steersman's sixth Wolf-child	Male				Wolf						X	X	X	
(17) Wolf-child of Shamparnur	Male	4			Wolf	X					X	X	X	
(18) Wolf-child of Shamparnur	Male	6			Wolf	X					X	X	X	
(19) Wolf-child of Shamparnur	Male	6			Wolf	X					X	X	X	
(20) Dina Samchar	Male	10		X	Wolf	Too weak			X		X	X	X	
(21) Second Wolf-child Sikandra, India	Male				Wolf						X	X	X	
(22) Second Overdyke wild-boy	Male				Wolf						X	X	X	
(23) Clemens of Overdyke	Male				Pig						X	X	X	
(24) Swiss-girl of Salzburg	Female	23			Pig						Crippled	Unintel- ligible	Fig-like	
(25) Wild-boy of Avonnet	Male								?		X	X	X	
(26) Wild-boy of Avonnet	Male								X		X	X	X	
(27) Half-wild boy of Zips, Hungary	Male	17							Half wild		X	X	X	
(28) Kaspar Hauser	Male	8									X	X	X	
(29) Anala, younger Midnapore girl	Female	3			Wolf	X		Dog			X	X	X	
(30) Kamala, older Midnapore girl	Female	8			Wolf	X		Dog			X	X	X	
(31) Leopard-boy of India	Male	5	3		Leopard	X		Dog			X	X	X	

TABULAR SUMMARY FERAL MAN AND CASES OF EXTREME ISOLATION—Continued

Case	Shuns humans	Completely selfish	Learned to be unselfish	Attached to person or feeds	Learned to like society	Inesaim to best and cold	Expresses anger and impatience	Ex-presses joy	Sym- pathy between wolf- children	Liked music	Degree of recovery	Number of words learned	Learned to do simple work
(1) First Lithuanian Bear-boy of 1661 ? (1677)											Little		When watched
(2) Hessian Wolf-boy 1344						X					Little		None
(3) Irish Sheep-boy 1673											Great	To talk	
(3a) Bamberger Cattle-boy											Great	To talk ?	
(4) Wild Peter of Hannover 1734											Little	To talk	
(5) The Pyrenees Boys 1719											Great		
(6) Girl of Gamenburg 1717											Great		
(7) Child of Sargol, 1731											No data		
(8) Jean of Liege											No data		
(9) Second Wolf-boy, Wetterau											Considerable		
(10) Second Lithuanian Bear-boy, 1694											Slight		
(11) Third Lithuanian Bear-boy											No data		
(12) Steerman's first Wolf-child	X			X									
(13) Steerman's second Wolf-child	X												
(14) Steerman's third Wolf-child													
(15) Steerman's fourth Wolf-child													
(16) Steerman's fifth Wolf-child													
(17) Steerman's sixth Wolf-child													
(18) Wolf-child of Sultanpur													
(19) Wolf-child of Shahjehanpur													
(20) Dina Samichar						X					Became policeman		To light a pipe
(21) Second Wolf-child Siharadra, India						X			X		None		A little
(22) Second Wolf-child Siharadra, India						X					None		
(23) Clemens of Overgrolke											Little		
(24) Swine-girl of Salzburg											None		Filled wa-ter-jug
(25) Wild-boy of Kronstadt							X			X	None		Chores
(26) Wild-boy of Aveyron		X					X				Little	30 words	Chores
(27) Half-wild boy of Zips, Hungary							X			X	Very great	Fluent	Chores
(28) Kaapar Hausar							X				Some	30 words	Errands
(29) Anna, young girl Midnapore	X			X		X	X	X			Some		
(30) Child of Midnapore girl	X					X	X	X			Some		
(31) Leopard-boy of India	X					X	X	X			Some		

SUMMARY AND CONCLUSIONS

In studies of human behavior and development, scientific experiment is often forced to take the direction of animal experimentation or the use of accidental cases approximating a controlled experiment. No scientist would dream of an actual experiment with a human subject under any such rigorous control as removing all human association and contact. Cases of feral man, and certainly the recent one of the Wolf-children of Midnapore, offer objective data, subject to this control, of fundamental importance to theories of human studies. Thus we may say that we have satisfactory evidence from far-away India of a crucial experiment made by a mother-wolf.

From his inadequate data, Linnaeus hit upon two of the constant features of feral behavior—*mutus* and *tetrapus*. All the cases of feral man agree with this one of Midnapore in being mute, often despite an acute sense of hearing. Most cases made animal-like sounds when recovered (see table.) Also, among animal-reared children there is but one case of recovery of human speech beyond the degree of recovery recorded of the Wolf-child of Midnapore, Kamala, of ability to say about fifty words. The failure of development of speech, the most characteristic human faculty, is of the greatest significance in the retardation of their mental faculties.

All cases of feral man reared by animals agree with the Midnapore cases and with Linnaeus' characterization, *tetrapus*, in the characteristic of walking on hands and feet or hands and knees, despite man's biped structure of limbs. In all cases reported, this biped structure is sufficiently modified by conditioning to a surprisingly rapid quadruped locomotion that it is only by protracted education that the erect position of man is attained for walking. In none of the cases of feral man, reared by animals, can they run upright; but, like Kamala, must take the four-legged position for rapid movement.

Despite the obvious gross human structure of feral man in brain and nervous system, we unfortunately have no post-mortem material on these Wolf-children of Midnapore or any other strictly feral cases. The only material that we have is on Kaspar Hauser. This is of paramount importance because he made a substantially complete recovery of human faculties and speech after severe and long-continued isolation. Yet his brain was undersized with an underdevelopment of the cerebrum and of the convolutions, approaching an impression of atrophy. Though the degrees of recovery vary from almost complete in the case of Kaspar Hauser to practically nothing in such cases as the idiot wolf-child, Dina Sanichar, the

initial human behavior of all cases appears idiotic, despite animal-like keenness of the senses of smell, hearing, and sight (the latter, especially at night), so often reported of feral cases.

The universal initial imbecility of feral man when regained to human society raises the question of throwing out the whole of the data as irrelevant to problems of human heredity and psychology, on the grounds that congenital idiocy is involved, if only because idiotic children would be more apt to be abandoned than normal ones. It would seem improbable, however, that out of some 30 creditable cases of feral man, pure chance should have operated to the result that all should be natural idiots. Further the case of Dina Sanichar, an obvious natural idiot, gives a datum in feral cases to work from.

Two other lines of consideration counter-indicate universal idiocy. First, several of the cases are of children who have wandered into the wilds, alone, and have survived to adolescence by their own wits and strength, unaided by human association, which would require considerable native intelligence. Secondly, in many of the cases of animal-reared children, as in the Midnapore case, there is the degree of recovery shown by all cases save Dina Sanichar, when in human society for some years. In the Rev. Singh's diary on the Wolf-children of Midnapore, one sees evidence of great stress and strain in the struggle of the elder child, Kamala, after the death of her younger human companion of the wolf-den. This struggle was to orient her affections and interests from animals to human beings. At first, she seemed about to sink into the gloom of unrelieved and despondent idiocy; but soon she began to take a glimmering of interest in her new surroundings and associations without the leadership of the younger and more plastic child, Amala, whose early death after a year in human contact was most unfortunate for the development of the elder girl.

The account reveals how the Reverend and Mrs. Singh were forced to treat both children when first rescued as newly born human babies. In the social, and in a very real sense, this is what they both really were upon their introduction into human society at the ages of one-and-one-half and of eight years of age. They had to be kept in bed, to be fed milk from a bottle and to be clothed only in a diaper-like breach-cloth. The breach-cloth had to be sewed on because they tore off clothing as savagely as animals would do.

This last reveals that their human handicap was greater than that of the difference between their chronological and mental ages. They had to unlearn all their animal behavior patterns and habits as well as shift their interests and sympathies from animals to human beings. Rev. Singh thought

that the first step was successfully accomplished when the wolf-children got over their craving for raw meat, on which, with animal milk, they had lived.

Their development of human behavior was slow due to the inhibitions of this animal patterning. The elder did finally learn control in evacuation and stopped tearing off her clothes. At long last, she came to demand her "fok" (frock) when company came or the children went out for a walk, though no color appealed to her except red.

The complete account of the Rev. Singh brings out clearly the difficulties in the reëducation of Kamala to the erect position in walking which took years and a complicated series of exercises. Yet her mastery of man's erect posture was so incomplete that for running Kamala, like other cases of animal-reared children, had to go on all fours. When she ran thus, it was hard for a person to overtake her.

In the complete account we see a development in language from animal communication to rudimentary human behavior in this essentially human faculty. Kamala mastered about 50 words which she was able to use in sentences of at least a subject and predicate. Thus the Rev. Singh's diary account of Kamala records the transformation of a human creature from an effective animal into a pathetic little sub-normal girl, who gave no one the impression of being idiotic; but one who became the favorite child in the orphanage where she was so fortunate as to live for nine years with many children of her own age under the kind care of the Rev. Singh and his wife.

If we speak of idiocy in many of these cases of feral man, we should follow Tredgold's concept of *isolation amentia* of Kaspar Hauser; or, in feral cases, in general, we should think with Rauber in terms of this idiocy as a *dementia ex separatione*, from isolation. As early as 1885 Rauber points out that the first years of human life are the most significant ones. Individuals subject during these years to isolation long and severe enough, recover from its effects to a degree only by an unusual strength and power of human plasticity.

Since natural or congenital idiocy does not appear involved in the instance of the wolf-children of Midnapore, as in some of the other instances, we are led to admit the relevance of these data of isolation to problems of human psychology and sociology.

Food and eating. Man, like other animals, eats; and both classes of feral man show a striking adaptability of the human organism to raw food both animal and vegetable, digested without preparation or refinement. All of the cases of children adopted by wild animals, wolves, and

bears, were sustained by raw meat, and nothing else, save milk, would be accepted at first recovery. After rescue these children exhibited the greatest craving for raw meat and would steal offal, carrion, etc. Tomko of Zips, Hungry, is reported as eating offal, as one of the Midnapore cases is shown doing in an actual photograph. Some of the cases that General Sleeman reported ate carrion at the edge of the villages with the dogs.

The cases of children who have been lost in the wilds to survive show even more strikingly the adaptability of the human organism for food requirements. Survival on bark, roots, grass, herbs, and leaves are recorded. Thus man appears to be able to sustain life as either an herbivorous animal or a carnivore. Several cases, if not all, are reported as suffering a severe illness soon after rescue. One cause of this, no doubt, is the change of diet. In the light of recent research in the effects of diet on the intelligence, there is but little doubt but that the inadequate diet of feral man has something to do with his mental retardation. There is, however, hardly enough evidence available to attribute it all to this cause.

In many of the cases reported, we have the animal behavior pattern of smelling food before eating. Such reports are those of the third Lithuanian Bear-boy (11), as well as Sleeman's cases (14) and (16). Other clear patterns of animal conditioning in eating are reported of Clemens of Overdyke (23) who, like the pigs he had lived with, went four-footed and rooted in the salad-beds until these were fenced against him. Another animal pattern of eating is seen in actual photograph of one of the Midnapore wolf-children drinking milk like a dog on all fours with the head down. This same thing is also reported for one of Sleeman's Indian wolf-children (13). Dina Sanichar (20) the Sikandra wolf-child, sharpened his teeth on bones, like a dog or wolf. It is noted of this child that the keeping clean of the cup and plate used in eating was one of the few human attributes he recovered. About the only other human behavior-pattern he mastered, besides the erect position in walking and the use of clothes, was the smoking of tobacco, a minor human vice of which he appears to have been notably fond.

Dina Sanichar's keeping his plate and cup clean, may be significant of the importance and power of eating in the shaping of human conditioning and the channeling of man's emotions. As with animals, feral man, *e.g.* Kamala (30), reveals that the first emotional attachments for human beings is to the person who feeds him.

Also the hunger motive is so impelling in human behavior that in a few of the cases of feral man we see only in his food habits a differentiation beyond that of the animals in the direction of the invention of a favorite

method of subsistence which appears as the rudimentary beginnings of human culture. The second boy of Overdyke (22) had learned to climb trees for birds' eggs and young which he could never be taught not to do. He was very skilled in climbing trees, as was the Wild-boy of Aveyron (26) who specialized in climbing for nuts and wild fruits. This reminds us of the Girl of Songi (7) who is recorded as expert in swimming for fish and frogs which furnished her favorite food.

Clothing. Other than this evidence of the strength of the hunger motive for the cultural beginnings for the food quest, we apparently have no evidence of other motivations in feral man strong enough for the invention of tools, houses, or the use of fire. Since feral man is reported as insensitive to ordinary climatic changes of heat and cold, any evidence of the use of clothes among feral children, that have grown up wild, has been interpreted as evidence of human contact rather than invention.

Contact rather than invention is indicated in the case of the Girl of Songi (7), who wandered back into civilization with a bottle-gourd on her head and with her body covered with rags and the skins of animals. Also the Cranenburg girl (6) is reported as having been captured wearing an apron of straw, as almost the sole relic of previous human contact. Human contact for both of these cases is also indicated by the considerable recovery reported for them. The Cranenburg girl, although not learning to talk, could soon pass for an ordinary person; while the Girl of Songi not only learned to speak, but regained so much of human personality as to become a nun, as we have already seen.

Less recovery is noted in the other two cases, those of boys, found wearing remnants of clothing as relics of human contact. Wild Peter (4), though naked when caught, had around his neck the last strings of a shirt, while the tanned marks on his thighs were convincingly adduced by Blumenbach as evidence that he had worn trousers and thus that he could not have been away from human contact for more than a year. The Wild-boy of Aveyron (26) showed evidence of human contact at no distant time before his final re-capture in the remnants of clothing apparently given him at some previous time.

In contrast to these cases of children who have wandered off wild in the forests, children reared by animals, when first caught, tear off clothing as savagely as wolves themselves might do. This is detailed in full for the Midnapore cases (29) and (30), from which we see their gradual conditioning to wear clothing, which ended in the pathetically human desire of Kamala to demand her "fok" whenever the children went walk-

ing, or visitors came to the Orphanage. Wolf-children savagely tearing off clothing at first and a long-continued disinclination to wear them is recorded for Sleeman's cases (12), (13), and (17), as well as the case from Shahjehanpur, (19), and Dina Sanichar of Sikandra, (20). It is recorded that the first Lithuanian Bear-boy (1) was forced by blows to wear clothes, a harsh treatment also accorded to some of the wolf-children.

Besides such difficulties in getting wolf-children accustomed to clothes, we read that Wild Peter (4) threw away his clothes, especially his cap, with glee; and that only after three years in captivity did the Kronstadt Boy (25) become accustomed to clothes, especially shoes.

Shame. That the wearing of clothes is small indication of the human sense of shame, so lacking in animals, is clearly indicated by primitive custom familiar to anthropologists. Such data as we have from feral man regarding a sense of shame suggests an infantile naïveté, which develops into the human attitude through social conditioning. The best account of this, as in the other cases, is that of the Wolf-children of Midnapore and that of Kaspar Hauser, Feuerbach says of the latter and a new case:⁸⁷

When once my wife and myself undressed him, in order to cleanse his body, he gave full proof of his innocence and ignorance; his conduct, on that occasion, was precisely that of a child; quite natural and unembarrassed. Not long afterwards, however, a feeling of modesty was awakened in him; and he became as bashful as the most chaste and delicate maiden. An exposure of his person he now regards with horror. After the wild Brazilian girl, Isabella, whom Messrs. Spix and Martins had brought to München, had lived for some time among civilized people and worn clothes, it was not without much trouble, nor yet without threats or blows, that she could be brought to undress herself that her shape might be drawn by an artist.

The straw apron worn by the Cranenburg girl (6) on her capture is evidence of a human sense of shame which probably, like the apron itself, derived from previous human contacts.

Sex. Whatever may be the relationship of the human sense of shame to sexuality, the latter, unlike the former, is a trait man shares with the animals and one which is closely linked with his biological endowment. Feral cases yield unexpected evidence of inhibition of the sexual impulse apparently due to unfamiliarity in the new situation and impossibility in the old.

While there are no special data on this head for the Girl of Songi (7), the fact that she became a nun might indicate that the sex impulse was not strongly developed in her. Tomko of Zips (27) is recorded as appearing

⁸⁷ Paul J. Anselm, Ritter von Feuerbach, *Caspar Hauser*. Translated by H. G. Linberg, 1833, 42 f.

"to feel no temptation to lust, despite the fact that he shows adequate sexual development. Once when herding sheep on a pasture, a girl tried to seduce him. He told about it afterwards with much disgust and repugnance." Similarly the accounts of Kaspar Hauser's life in human society, almost exactly during the period of adolescence from the ages of 17 to 23 years, during which almost his every act was observed and recorded, are devoid of anything to indicate other than that sex was dormant.

There are hints in the diary on the Wolf-children of Midnapore that Kamala (30), before her death at 17 years, showed diffuse and unpatterned sex expressions, which were also indicated for the Wild-boy of Aveyron (26). The account of the Wild-boy of Kronstadt (25) traces more clearly the emergence of unpatterned sex expression, unchanneled through a complete lack of proper association:

Probably it was his complete unfamiliarity with his new condition . . . (that) he showed not the slightest emotion at the sight of a woman. (Wagner's foot-note) In this respect is it surprising that he showed no adjustment? For how could this wild boy of Kronstadt, like Wild Peter of Hameln, know anything about women? They saw in a woman nothing else than a human being dressed in clothes different from those of a man. This question of their interest in woman could only be solved if they had seen and been in communication with human females freed of their cultural accoutrements. The following proves this observation. When I saw him again after three years, this apathy and disrespect had disappeared. As soon as he saw a woman, he broke out into violent cries of joy, and tried to express his awakened desires through gestures.

Wild Peter, just mentioned, lived to the ripe age of 80 years without any such behavior being recorded for him; but this does not disprove Wagner's thesis.

The evidence of the Wild-boy of Kronstadt shows the emergence of the sex impulse at puberty, causing restless and unpatterned behavior, as in the Wild-boy of Aveyron, who lived so long that he sank back unexpressed into dormancy from which in Kaspar Hauser and Wild Peter it appears never to have emerged. These data indicate that the social channeling of sex is much more necessary for its full expression than might be thought. Certainly this preponderating evidence of the inhibition of the sex impulse through isolation from social contact is surprising, especially considering the other gross animal behavior of feral man in other urges, like hunger.

Sensation. Sensation is a fundamental psychic phenomenon, apparently closely linked with the biological endowment of man, since highly developed sensory organs go back in the scale of life as far as such molluscs as the octopus and the squids. The sensory perceptions of feral man show surprising conditioning to their animal or other non-human experience.

Both classes of feral cases show striking ability to see, especially at night. The Wolf-children of Midnapore (29) and (30) were able to see surprisingly well at night. They prowled "like wolves" as did also the second Sikandra boy (21), and Sleeman's second wolf-child (13).

The conditioning of Kaspar Hauser, having been as long as he could remember in a dark dungeon, resulted in visual prowess that is as much more extraordinary as our account is more complete and detailed. On the darkest nights he could walk with certainty, and was amused by others groping for the stairs or gates.

The sense of smell of feral man, when reported, is described as sharp. One of the Wolf-children of Midnapore located by smell the fresh entrails thrown out with the garbage. Sharpness of smell in feral cases is indicated by the frequent report of smelling all food before eating it. This is recorded for the third Lithuanian Bear-boy (11), the Irish Sheep-boy (3), the two Sikandra Wolf-boy (20) and (21), and Sleeman's similar cases numbered (15) and (16). Of Jean of Liège (8) about all that is recorded of his behavior after his rescue from the wilds, other than his subsistence on wild vegetables, is that his sense of smell was extraordinarily sharp; and, even at a distance he could recognize his female guard by her odor. In time, according to account, he lost this sharpness of smell as he became accustomed to our common food.

The sense of hearing of none of the cases of feral man is reported as defective. Our better records, such as the diary of the Midnapore Wolf-children, show that feral cases have had an unusually sharp sense of hearing. Also the excellent account of the Wild-boy of Aveyron records that he responded to the sound of the cracking of a nut, to the eating of which he had been accustomed when wild. Sleeman's fifth wolf-child (16) "heard exceedingly well." It is thus not only quite clear that the mutism of feral man is not due to defective hearing, but also that feral cases have improved their hearing by animal conditioning—the only sensory improvement noted.

There are few data in the records relative to the sense of touch in feral man, but there is frequent note of a surprising insensitivity to hot and cold. The Wolf-children of Midnapore agree with the records of the wolf-reared children, (12) and (19), in their appearing insensitive to the annual shift in temperature in Northwest India, and refusing covers on the coolest of nights. The same is reported by Tulp (whose portrait we have from the brush of Rembrandt, as the demonstrator in the well-known painting, "The Anatomy Lesson") for the Irish Sheep-boy. The Wild-boy of Aveyron is recorded in Rauber as so insensitive to hot and

cold as to have taken potatoes out of a pot of boiling water, soon after his capture. Anthropologists are familiar enough with records of similar exposures of the hands in primitive ordeals.

The best account of the sensory perceptions we have is of Kaspar Hauser, and is one that deserves detailed attention from psychologists interested in the development of sensory perception. The inadequate accounts of the true cases of feral man give no data inconsistent with the following discussion by Feuerbach of his observations on Kaspar Hauser, penetrating for 1833:

The most remarkable fact of experience in respect to him, which I learnt, but which was not fully explained to me until several years afterwards, was the result of the following experiment, which was suggested to me by a very obvious association of ideas, leading me to compare what was observable in Kaspar, who had not come forth from his dark dungeon to the light of day before the age of early manhood, with the well-known account, given by Cheselden, of a young man who had become blind but a few days after his birth, and who, in consequence of a successful operation, had been restored to sight at nearly the same age (as Kaspar Hauser).

I directed Kaspar to look out of the window, pointing to the wide and extensive prospect of a beautiful landscape, that presented itself to us in all the glory of summer; and I asked him whether what he saw was not very beautiful. He obeyed; but he stantly drew back, with visible horror, exclaiming, "Ugly, ugly" and then pointing, to the white wall of his chamber, he said, 'There are not ugly.' To my question, "Why was it ugly?", no other reply was made, but "Ugly, ugly." And thus nothing remained, for the present, for me to do, but to take care to preserve this circumstance in my memory, and to expect its explanation at the time when Kaspar should be better able to express what he meant to say. That his turning away from the prospect pointed at could not be sufficiently accounted for, by the painful impression made upon his optic nerve by the light, appeared to me to be evident. For his countenance at this time did not so express pain as horror and dismay. Besides he stood at some distance from the window, by the side of it, so that although he could see the prospect pointed at, yet, in looking at it, he could not be exposed to the impression made by the rays of light entering directly through the window.

When Kaspar, afterwards, in 1831, spent some weeks with me, at my own house, where I had continual opportunities of observing him accurately, and of completing and correcting the results of former observations, I took an opportunity of conversing with him respecting this occurrence. I asked him whether he remembered my visit to him at the tower; and whether he could particularly recollect the circumstance, that I had asked him how he liked the prospect from his window, and that he had repeatedly exclaimed, "Ugly, ugly" . . . I then asked him, why he had done so? and what had then appeared to him? To which he replied, "Yes, indeed, what I then saw was very ugly. For when I looked at the window it always appeared to me as if a window-shutter had been placed close before my eyes, upon which a wall-painter had spattered the contents of his different brushes, filled with white, blue, green, yellow, and red paint, all mixed together. *Single things, as I now see things, I could not at that time recognize and distinguish from each other.* This was shocking to look at; and besides, it made me feel anxious and uneasy; because it appeared to me as if my window had been closed up with this parti-colored shutter, in order to prevent me from looking out into the open air. That what I then saw were fields, hills, and houses; that many things which at that time appeared to me much larger, were, in fact, much smaller, while many other things that appeared smaller, were, in reality, larger than other things, is a fact of which I was afterwards convinced by the experience gained during my walks; at length I no longer saw any thing more of the shutter." To other questions, he replied, that in the

beginning he could not distinguish between what was really round or triangular, and what was only painted round or triangular. The men and horses represented on sheets of pictures appeared to him precisely as the men and horses that were carved in wood; the first as round as the latter, or these as flat as those. But he said, that, in the packing and unpacking of his things, he soon felt a difference; and that afterwards, it had seldom happened to him to mistake the one for the other.

Emotions. One thinks of certain of the emotions as closely linked with the biological inheritance of man. The inhibition of sexual expression has already been noted. The other data on the expression of emotion in feral man show the expression of violent emotions of anger and impatience, not only for the Wolf-children of Midnapore, but also for the half-wild Tomko of Zips, Hungary, the Wild-boys of Aveyron and of Kronstadt, and the idiotic Wolf-child, Dina Sanichar.

So human an expression of emotion as laughing, or even smiling, is not recorded for feral cases. The Rev. Singh well considers it near the crisis of the quest of Kamala for humanity, when one tear fell from her eyes in grief at the death of Amala, as herein recorded. As for any observable effect of music on the emotions of feral cases, this was great in the case of Kaspar Hauser, as is in keeping with his greater sensitivity. Wild Peter used to jump gaily in rhythm of music, whereas the Wild-boy of Kronstadt only listened to tunes on the piano with apparent pleasure. His slight interest in music did not, however, leave any impression of it with him, we are told. No other data are given.

Social aspects. The material of feral man deals a hard blow, as the Rev. Singh specifically points out, to the "social instincts" of gregariousness, or to what Giddings called the "consciousness of kind." The Wolf-children of Midnapore were noted to shun human society at first almost as completely as a wolf might have done. This is also recorded by Sleeman for his first and fourth wolf-children (12) and (15). More than shunning human society, several cases of feral man, besides the Wolf-children of Midnapore, are reported as preferring the society of animals, the more closely related to the species of their prehuman associates, the better. Any animal, however, was preferable to humans with the Midnapore children. They followed the chickens; and Kamala was helped in walking by the strengthening of her legs in imitation of the climbing of a cat, when she wouldn't follow human instruction at all.

Other animals associations preferred to human society are reported of feral cases. Kaspar Hauser loved horses and played with hobby-horses hours at a time in utter unconsciousness of human presence. Clemens of Overdyke preferred pigs to human associates; and Sleeman's cases, (12) and (17), loved dogs in lieu of their foster wolf parents. More extraor-

dinary is the additional report in the last case, as well as in that of one of the Lithuanian Bear-boys, that the wild animals involved lost their fear of man in the presence of the feral children. Rev. Singh had the insight to keenly regret having to kill the wolf-mother of the children of Midnapore, observation of which would have been very interesting on this point.

Even greater sympathy than between feral children and wild or other animals is the strong bond of sympathy between pairs of wolf-children when they have occurred together as at the Midnapore and the Sikandra Orphanages. All other feral cases, like Kamala after the death of Amala, become attached at first only to the person who feeds them. A further impression of feral cases, as they counter-indicate an instinctive consciousness of kind, is that they are completely selfish, though definite statements for this are lacking except for the Wild Boy of Aveyron. In the present record of Kamala we have an excellent, and the only detailed, record of the shift in the emotional ties of a feral case from animal to human associates. Thus Kamala came to like human society and missed Mrs. Singh greatly during a long absence.

Thus defective are the most elementary emotional responses of feral man in these most rudimentary human traits of socialization. Feral man has the sensory and other biological equipment that man shares with the animals. The biological foundation is there; but the human superstructure is vastly lacking, when the children are found. Perhaps half of the cases show a noticeable recovery which may be indicated by the number of words learned, varying from 30-50 in some cases to a much more considerable recovery shown by a more or less fluent mastery of one or even two languages, seen in the cases of Tomko of Zips and Kaspar Hauser. Of these recoveries, however, only Kaspar Hauser gained a real mastery of the essential patterns of human personality. Even about him, there is from all observers, a sense of tragedy that he had been so irreparably harmed by the "crime against a human soul" that had been committed against him. Certainly Kaspar Hauser and the cases of feral man have the greatest significance to students of human psychology.

For experimental control psychology may well study man as a set of variables while assuming or seeking the environment as a constant, leaving to sociology the variables of the social environment while it assumes the individual as a constant. These data of the 'controlled' experiments of feral man approximate such a set-up.

Man is born with a neutral organization unique in the biological realm, yet apparently the individual has to live with other human beings in order to enter into his human heritage. Radicals from the days of Rousseau have

prejudiced the scientific case for feral man by the fervor of their espousal of these cases as proving that the environment completely molds the human mind, and mentality. They overlook that mentality is a bioneurological mechanism, and mind is the environmentally conditioned content organized by that mechanism. Though in the main part of this study we have seen well-attested cases of human beings reduced to wolf-conditioning, the radical thesis still needs a case of a wolf raised to human behavior and language.

Meanwhile we shall continue to envisage human heredity and the psychological factors as paramount, with the environing factors operating with them in a complex interplay. It would appear clear that, within the complex organism of human neurology, man's faculties and capacities arise to function, but they need the environmental stimulus as seeds need the good earth. Deprived of the earth the seeds die, and so human faculties atrophy when deprived of the environmental stimulus of language, thought, and human association. By animal association this human equipment can be conditioned to animal behavior, locomotion, food-habits, and emotional response. Deprived too long of human association, or animal-conditioned too strongly, the sensitive potentialities of human development are permanently inhibited and the traces of animals conditioning are never completely lost.

Sociology, which investigates man's society for its significant variables in the conditioning environment, has long been interested in the data of feral man as crucial. This material, while in no way invalidating the hereditary and bio-psychological factors, does attest most emphatically to the validity of the realm of social conditioning as a field for the investigation into some of the most significant problems of human behavior.

Sociology studies and delimits the social realm as a super-organic structure which provides the conditioning environment of the individual from the day of his birth. That human society is a structure or organization with the most profound of effects upon the individual could not be more definitely shown than by the data on the Wolf-children of Midnapore, and to a less degree by the other cases of feral man, here summarized.

Cultural anthropology has added a valuable concept to social science in its conception of culture. Just as society is the environing structure or mechanism, so culture is the corpus of most of these patterns of human behavior passed on through the social mechanism. It is precisely these patterns of behavior which feral man lacks.

This technical concept of culture is different from the ordinary use of the term in such phrases as "a person of culture" or "a cultured man."

In such usage, culture means simply having education, moderated conduct, and a marked set of manners which connotes a sophistication and a depth of background of a certain sort. Anthropologists have found it useful to widen this conception to include the results of all human association and background.

By culture, the anthropologists mean that enormous part of human thought, belief, and behavior that results from human contact and association with others. These feral cases show how much the individual owes to the stimulation of contacts with and ideas from others. Thus is indicated how important a thing culture is in this sense, as a repository of all that has gone before which has found human acceptance.

As culture is the super-organic repository of all that which has gone before to find acceptance in human conditioning; and, as sociology is the study of the social structure, environment, and interactions of men whereby this heritage is taken over, so social psychology is the synthesizing science between these social and cultural fields on the one hand, and the psychic sciences of man as an individual on the other.

Social psychology occupies this intervening field and effects a necessary methodological simplification by assuming man as the 'individual,' to be a biological mechanism. For social psychology this bio-psychic mechanism is assumed as a constant to be investigated by the psychologist.

From this assumption the social psychologist investigates man as the 'personality' in a technical sense referable to the *personae*, i.e. the masks or rôles that the 'individual' is conditioned to wear before his fellow-players in society. That posture, locomotion, sexual expression, and food habits are subject to such patterning, and language and culture the products of it is shown by the data on feral man, which clearly reveal the reality and significance of these *personae* patterns or rôles in which the 'individual' participates with his fellows from the time of his birth. In feral cases, such as those of the Wolf-children of Midnapore, association with wolves gave these two Indian girls the effective conditioning of wolves sufficiently strong as to have inhibited human behavior. Nine years of careful education in human society, in which most of these animal behavior patterns were put aside, were necessary before the elder surviving child had attained sufficient familiarity with human *personae* patterns to develop in her the essentials of human personality (the rudiments of language, the use of clothes, and the upright position). Unhandicapped by either animal conditioning or that of survival isolated in the wilds Kaspar Hauser, appearing in the world at 17 years of age as a *tabula rasa*, had neuro-

logical equipment sensitive enough to go far beyond the others to the attainment of a well-nigh complete familiarity with the *personae* patterns current in Bavaria of his own day.⁸⁸

⁸⁸ Contemporary cases like that of Kaspar Hauser, of isolation by cruel guardians, so similar to the isolation in feral cases, are under investigation by American scientists. Kingsley Davis' study of a Pennsylvania girl (Extreme isolation of a child, *Amer. J. Soc.*, 45, 1940, 554-565) does not reveal much recovery of human personality, but another case in Ohio, being studied by F. N. Maxfield of Ohio State University, is showing great improvement. It is worthy of note that Davis' conclusion, which I did not see during the preparation of this paper, also refers to *personae patterns*. He writes, "Anna's history, like others, seems to demonstrate the Cooley-Mead-Dewey-Faris theory of personality; namely, that human nature is determined by the child's communicative social contacts as much as by his organic equipment" (*op. cit.*, 565).

A STUDY OF THE WAY IN WHICH A VERDICT IS REACHED BY A JURY

By H. P. WELD and E. R. DANZIG, Cornell University

Little is known about the manner in which the evidence in a court-trial is received, weighed, and accepted by a jury. Weld and Roff have made a study in which a large number of individuals listened to the report of the evidence in a criminal trial and, as individuals, reached a verdict concerning the relative guilt or innocence of the prisoner. Since the witnesses themselves were not heard in either direct or cross-examination, since there were neither opening nor summing up statements by the attorneys, and since the individual subjects had no opportunity of discussing the case with each other, no claim was made that the experiment showed what would happen in an actual trial.¹

In the present study we were able to overcome these omissions, to some extent at least, by availing ourselves of a trial in a moot court, one of a series conducted by Professor Lyman Wilson of the Cornell Law School. The case was prepared by Professor Wilson who also acted as Judge.² The counsel for the plaintiff and the defendant were students in the Law School, and three juries were selected from students who were taking courses in psychology. All of the proceedings were recorded by a professional court stenographer. The trial was held in the Moot Court Room of the Cornell Law School on two successive Tuesday afternoons, April 9th and 16th, 1940. The evidential details of the case were presented by witnesses who had been selected and coached by the attorneys.

The entire trial was conducted in the manner of a real trial, with several minor exceptions. The most important of these were that the jurors were not examined and no oaths were administered. The excellent acting by the witnesses, the introduction in evidence of photographs and blueprints, and the earnestness of all participants in the trial gave an air of reality to the proceedings such as to simulate an actual suit at law.

The three juries consisted (A) of twelve men, another (B) of twelve women, and a third (C) of seventeen men and women.³

* Accepted for publication June 13, 1940.

¹ H. P. Weld and M. F. Roff, A study in the formation of opinion based upon legal evidence, this JOURNAL, 51, 1938, 609-628.

² We are grateful to Professor Wilson for his hearty cooperation. It was his sympathetic interest and cordial support that made the experiment possible.

³ This jury originally had 24 members divided into 2 juries of 12 each, but 7 members were unable to attend throughout the trial.

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METHOD AND PROCEDURE

The trial in the courtroom was divided into 18 stages, which hereinafter will be indicated by roman numerals. Every member of the jury was given a data sheet on which the stages were printed in order and the juror was directed to record his judgment for the plaintiff or defendant in terms of a 9-point scale, after each stage had been presented to the court. This scale was as follows: 1, conviction that the defendant is not liable for damages; 2, strong belief that the defendant is not liable; 3, fair belief that the defendant is not liable; 4, slight belief that the defendant is not liable; 5, doubt that the defendant is liable; 6, slight belief that the defendant is liable; 7, fair belief that the defendant is liable; 8, strong belief that the defendant is liable; 9, conviction that the defendant is liable.

After the judge had finished his instructions to the jury, the three juries retired to separate jury rooms under charge of a foreman who previously had been selected as one having prestige with his particular group. On reaching the jury room the foreman immediately instructed every juror to record on his data sheet his vote or judgment on the following statement: "I find by a preponderance of the evidence that the defendant is liable." After the judgments were recorded the foreman polled the vote. The foreman had previously been instructed that if the poll was not unanimous he should make sure that the points at issue as expressed by the judge were clearly understood by every member of the jury, and then proceed to a discussion. When he thought it advisable he could take a second poll in the same manner as the first, and further polls as necessary. The jury was to be dismissed if unanimity was not reached at the end of thirty minutes.

The case ^{skp to 518} [A paraphrase] of the case comprising the opening and closing statements of the attorneys for both parties, the testimony, and the judge's charge to the jury, as divided into installments, is as follows: (B)

1. *Opening statement of the plaintiff.* Twenty years ago the plaintiff and her husband were married. During their honeymoon they had spent a few days at the Bear Mountain Inn, a well known summer resort located near Hamburg, State of York Island. They then returned to the town of What Cheer, Iowa, where they have resided ever since. Last June on the twentieth anniversary of their marriage they decided to retrace the route of their honeymoon journey. In so doing they naturally arrived at the old Bear Mountain Inn. Upon parking their car, they walked into the lobby of the building, which was empty, and stepped up to the desk. A young man appeared behind the desk and offered to accommodate them. The plaintiff and her husband did not register, but the young man said that they could have rooms in about two hours. The plaintiff had with her some jewelry, a diamond bar-pin and a platinum bracelet, and four hundred dollars in cash. Not wishing to carry these valuables around with her, she asked the young man if there was a place where she could safely put them. He told her that he would put them in the safe, and thereupon, she handed the jewelry and money to him, and received a receipt for them. When they returned to the desk about 6 o'clock to claim the room they had reserved, the clerk, a different man, informed them that he knew nothing about either the room or the valuables. The manager appeared and disclaimed all responsibility when the plaintiff demanded that the loss be made good.

Attorney for the plaintiff stated further: that the Bear Mountain Inn had five

years earlier been sold to the defendant; that the defendant claimed that they ran the place as the Bear Mountain Club, a private organization, and not as a hotel open to the public; that the defense rests upon this ground and upon the ground that the plaintiff had no right to come upon the Club's grounds and that hence the Club owed the plaintiff no duty of care.

Plaintiff will prove that the defendant continued to operate the Bear Mountain Club as a hotel and so is liable for the plaintiff's loss, on the theory of innkeeper's absolute liability; that even if defendant is not responsible to plaintiff on this theory, the defendant in fact held the place out to the plaintiff as a hotel and in so doing owed a duty of care which, by allowing a stranger to appear behind the desk of the club, it failed to meet.

II. *Opening statement of the defendant.* The facts as laid out by the plaintiff were substantially correct. The issues in this case are: (1) Is this a hotel or a club? The defendant will prove that it is not and it cannot be a hotel. (2) Did defendant hold itself out as a hotel? The defendant will show that this Club did all that was in its power or that could reasonably be expected of such a corporation to give the appearance of a club.

The defendant will also prove that the plaintiff, as a reasonable person, could tell this was a club and not a hotel; that the place had changed to such an extent that the plaintiff should have been put on her guard; or that the plaintiff was so contributorily negligent as to bar her recovery.

III. *Ella Meeker. First witness for the plaintiff (direct examination).* About 3 o'clock, June 4, 1939, as she and her husband approached the village of Hamburg in a car driven by Mr. Meeker, he asked a man the direction to Bear Mountain Inn. They received directions and proceeded slowly to the Inn. Upon arrival they noted that the general appearance of the place was much as it was when they were there 20 years before. After parking the car they entered the lobby of the Inn, which was empty. A young man came through a door, which was apparently unlocked, behind the desk and they told him they wanted a certain room on the third floor in the east wing, which they had formerly occupied twenty years previously. The young man said that the room was at present occupied but that the guest was expected to leave about 6 o'clock that evening and they could come back then and have the room. Witness asked if in the meantime she could deposit her valuables and the young man assented. She laid her brooch and bracelet and \$400 in cash on the desk and the man gave her a receipt (which she identified and which was put in evidence). She gave Mr. Meeker the receipt and they both walked outside and wandered over the place, returning two hours later, at 6 o'clock. A different man was now behind the desk, and he claimed to know nothing of the room reservation or of the other clerk. He called the manager, who was surprised at Mr. Meeker's questions about the room and asked if the Meekers did not know that this was a club, not a hotel. Mrs. Meeker asked for her jewels, but the valuables could not be found, and the manager refused to make good the loss. Mrs. Meeker identified a blueprint plan as a correct representation of the lobby, and it was put in evidence.

IV. *Ella Meeker. First witness for the plaintiff (cross-examination).* She and her husband had stopped at hotels on the previous two nights, but on neither occasion had she checked her valuables, thinking it unnecessary as she kept them with her

always. She did not remember the road to the Inn from Hamburg, but she remembered the appearance of the Inn, which she had not seen in 20 years. As they turned in toward the Inn, she saw a sign saying Bear Mountain Inn, on the left side of the road. She saw no other sign. She saw no throngs of people around the hotel. It did not strike her as unusual that there was nobody behind the desk when she came in. She did not remember whether anyone was behind the desks in the other two hotels at which they stopped. She did not remember the number of the room she occupied 20 years ago, but she recalled that it was in the east wing on the third floor because her husband had talked with her about it several times. She had read the receipt very carefully. The fact that the words, "Bear Mountain Club," were printed under the signature of the first clerk raised no suspicion in her mind. She noticed no signs over the desk. She did not register. Although there was some sort of golf tournament going on, she did not see many people. The clerk who took her valuables was young and presentable, whereas the clerk who was behind the desk when she returned was a different type of man, whom she could identify but did not now see in the courtroom. The first clerk did not say that this was a hotel nor did he say it was a club. She did not understand the term in her complaint, "domestic membership corporation," although she had made a sworn statement that everything in her complaint was true. She understood it at the time she signed it because her lawyer explained it to her then. Her eyes are good, but she saw no little sign under the big one at the entrance to the hotel.

V. *Philip Meeker. Second witness for the plaintiff (direct examination).* On June 4, 1939, he and his wife, on their way to "Bear Mountain Inn or Club," where they had been once before, 20 years ago, "asked a person on a street in Hamburg to direct them to Bear Mountain Inn or whatever it is." Following the directions, they reached the driveway, which had a couple of brick posts on either side, the one on the left bearing a sign reading, as he said, "Bear Mountain Club or whatever it was." They were going about five miles an hour at this point, and Meeker saw no other sign. The building looked much as it had 20 years ago. He parked in the parking lot to the right, and went inside with Mrs. Meeker. There was no one around the hotel, no doorman met him, and no one was in the lobby. That did not strike him as unusual, because it was warm, and nobody sits around hotel lobbies on warm afternoons. No one was at the desk, which was in the corner with a paper and cigar counter, but in a couple of seconds a man pushed open a door from the hall back of the desk. They asked him for a room on the third floor, east wing. Witness remembered the location because when he was there last the sun came in the window in the morning and woke him up. They did not sign the register, but he did not think it strange, as at other hotels they first assign a room to you and then have you sign when they give you the key. The clerk said the room was in use at the time, but would be vacated by 6 o'clock or thereabouts. They thought that would be all right. Witness had been talking with his wife about checking the valuables she had. She had not checked them at other hotels because she wanted to wear the brooch and bracelet, but she did not expect to need them here, as they did not think the Inn was very formal. Mr. Meeker suggested that she check them, so she asked the clerk if she could. He drew out a piece of paper and started to make out a receipt. He took the articles and counted the money, and made out the receipt as if that was his regular procedure. They then went out one of the side

entrances, crossed the terrace and walked down along the bridge path. When they came back to the desk they found another person there. Meeker asked if the room was ready, as the other clerk had promised. This clerk seemed surprised, and said he was the only clerk around there. Then Meeker told him about checking the valuables, and he knew nothing about it. The manager came over and joined in the discussion, and a few people collected. As the Meekers were about to leave a man came up and introduced himself as Frank Fleharty.

VI. *Philip Meeker. Second witness for the plaintiff (cross-examination).* When he asked for directions, he asked about Bear Mountain Inn, not, as the cross-examiner said, "Bear Mountain Inn, or Club or whatever it is." The fact that the sign on the post said Bear Mountain Club did not arouse any suspicion, especially since he recognized the building. It did not occur to him that ownership might have changed in the 20 years since he was there. He left his baggage in the car because there was no doorman there. This fact did not appear strange because country inns do not have doormen. He did not carry his luggage in because usually the clerk acts as a jack-of-all-trades and comes out afterward to get it. The sign reading "Club," the lack of doorman, and the absence of a clerk did not cause him to think it was other than an inn. He did not think anything about it, and the clerk appeared almost immediately. It would have been possible for the first clerk to have inserted a key and unlocked the door leading to the desk without Meeker's knowledge. Witness does not know who checked his baggage when he came there 20 years ago. His wife had no valuables to check at that time. He does not always leave it to his wife to do the checking, but these were her valuables and Meeker was right there. Although his eyesight is good, he did not see any signs in the hotel. He had testified that "we asked for the things we had checked" because, although he had not checked anything, they were together, and he had the receipt. Mr. Meeker and his wife had no food or refreshments at the Inn. For sentimental reasons he wanted the same room he had had before.

VII. *Frank Fleharty. Third witness for the plaintiff (direct examination).* He is a bond salesman living in New York City. On June 4 he was at Bear Mountain on his vacation. He came into the Inn about 5 o'clock. He has stayed at Bear Mountain every summer for the last 10 years, sometimes three, sometimes four weeks. He is not a member of the club and he has not a guest card. He knows of several persons that are neither members nor guests who stay there. In the only case he knows of when a person lacking a membership or guest card was turned away, the people were of the colored race. He was familiar with the place when it went under the name of Bear Mountain Inn and he attributes the bankruptcy to the undesirable clientele which kept the better class of people from patronizing the place. In the village of Hamburg, with which he is quite familiar, the natives always refer to it as the Inn. He has never heard it called a club in town. There is a sign over the desk which has to do with restrictions to people with membership cards or members only. It is inconspicuous, hanging among trophies on the wall. He thought the photograph here entered in evidence was a fair representation of the entrance to the club grounds. (In the photograph there is a sign saying "Bear Mountain Club," on a brick pillar, and to one side, almost covered by bushes, is a small sign which says, "for members only."). He had never noticed the small sign.

VIII. *Frank Flebarty. Third witness for the plaintiff (cross-examination).* There is good fishing around the inn, which is why he has been coming back for 10 years. He had heard about the bankruptcy, but he expected no difficulty and went back in his old standing. He knew people who frequented the club but he had no membership list. He knew many town people. The guests are mostly from a distance, but some are from town. He knows people who come to the inn who do not belong to the club, and some who do belong. He does not know if they have membership cards; but he has a good friend who has been admitted although not a member. He does not think his friends in town helped him get in after the club was formed. He does not know whether anyone could stay at the inn without being a member of the club, but he knows he did and his friend did. He does not know whether his friend knew club members or not. The people who in direct examination he had said had been turned away, had been refused because of their race. He had said that the hotel went bankrupt because the clientele was becoming undesirable, yet he continued to go there because he wanted to fish. He was a member of this clientele. The townspeople think the inn is a hotel, they refer to it as an inn, and they think they could go there and get in at any time. He never noticed the little sign that is in the picture. He is not familiar with photography, but he would say that the photographer was standing about in the middle of the junction of the drive and the road when he took the picture. The big sign is on the front of the post facing the road. The driveway is made of gravel and goes off at about a ninety degree angle. He had never met the plaintiff before the day in question.)

IX. *Richard Howard. First witness for the defendant (direct examination).* He has been employed by the club for almost five years; it has been a club throughout this period. He is clerk and general handy man at the club. He sells candy and cigars, arranges for golf and tennis lessons for members, and sees that the guests are taken care of. The club is for members and their guests and each of the guests is given a guest card. If a stranger should come with a member, he would ask the latter if the stranger was his guest. If the stranger was not the guest of the member, he would ask him to leave. If the stranger established himself as a guest through his guest card, he would remember him for a reasonable time after that. His other duties take him away from the desk frequently, so that he is around the grounds, is called out every 15 or 20 minutes. On June 4th at about 3:45 P.M., he noticed that the players in the golf tournament were getting on the 18th green so he went to see that the trophies were there. He was on and off the golf course all afternoon. The place was so crowded with spectators that he can not see how anyone could miss seeing that something unusual was going on. There was no reason why he should not leave the desk, as nothing of value is there. After he had been at the desk for an hour, around 6 o'clock Mr. and Mrs. Meeker came in and demanded a room that they said the other clerk had reserved, but he told them he knew nothing about it and that there was no other clerk. He asked to see their membership or guest card and they said they did not know one was needed. Witness explained that this was no longer an inn, and Mrs. Meeker said "all right" and asked for her valuables. She became incensed when he told her he knew nothing about them. He called the manager, who also explained that guest cards were required. A crowd gathered and finally the Meekers left. Mrs. Meeker said nothing about thinking this was a club. The club does not sell rooms. There are rooms for the convenience of

members and they are charged a slight fee, but it is restricted to members and their guests only. Sometimes someone would come and spend the summer at the club. This has happened within the past five years. There has been no trouble at the club similar to the present situation. Witness never takes valuables from the guests to keep for them since they have lockers which are rented for that purpose. They are in the locker room, and witness has nothing to do with them. There is no place for him to keep valuables for the guests.

X. *Richard Howard. First witness for the defendant (cross-examination).* He has been working at the inn about five years, works from seven till eight, and finds the job healthy and not too confining. He works there only in the summer and he does not think he works too hard. He usually goes to the movies when he has extra time.

XI. *Thomas Wheeler. Second witness for the defendant (direct examination).* He is steward or manager of the Bear Mountain Athletic Club. When he was called to the desk and Howard explained the situation to him he told Mrs. Meeker that they were not liable because this was a club, and he mentioned the sign which said "Bear Mountain Club." She thought it said "Bear Mountain Inn." She was not certain whether she had given her purse to Howard or to another man. When Wheeler told her that he would search for the purse and mail it to her if it was located, she went away apparently dissatisfied. A diligent but unsuccessful search was made, and witness wrote Mrs. Meeker to that effect. Howard told him that he had not been at the desk. Howard's duties required him to be on the grounds at times. The club has a certain number of members who have cards, and both Howard and witness know all the members on sight so there is no trouble when members come. The members can bring guests who are given guest cards. It is possible but improbable that an outsider could get lodging there without a guest card. It might be, as happened with Mr. Hummel once, that a member would come in with two or three guests and witness would fail to supply them with guest cards. The picture which plaintiff offered in evidence is not a correct picture of the entrance as it looks now. Neither of the two signs shown is now present. There is instead a rustic sign made out of wood in an arch over the road between the two posts. The sign says "Bear Mountain Club" and underneath the sign says "For Members Only." The driveway is approximately 12 or 14 ft. wide between posts and the sign stretches across the whole distance and stands probably $2\frac{1}{2}$ or 3 ft. high. The letters are $2\frac{1}{2}$ to 3 ft. high. The picture shows the entrance as it was five years ago, when the road was graveled. A similar picture was taken in order to get the trustees to appropriate money for a new sign. The road was paved four years ago. Witness remembers because that was when he got a new car and got tar on it.

XII. *Thomas Wheeler. Second witness for the defendant (cross-examination).* The sign was put up because formerly the place had been an inn, and with the change to a club witness thought it necessary to put the sign up. There has been a sign stretching from post to post for the last four years. He tries to keep the grounds restricted to members and guests. The Bear Mountain Club sign is in an arch with letters 2 ft. high. The "For Members Only" sign is considerably smaller, directly beneath the larger sign. It did not occur to him to bring to court a photograph of the new sign to refute the photographic evidence of the old sign submitted

by the plaintiff. He does not think this case will affect his job, though he has an interest in the club itself. The club has been involved in other law suits in the last five years. He was on the grounds near the golf course all afternoon but he did not walk around and look at the sign. He did not look at it the day before. He rooms at the club and goes out often. The sign is not lighted up, so that at any time it would be possible for someone to enter without seeing the sign. He could not say when he can remember reading is consciously.

XIII. *Samuel Hummel. Third witness for the defendant (direct examination).* His home is in Hamburg, and five years ago when Bear Mountain Inn failed he was one of three members of a committee, from a sort of athletic club, which purchased the inn at the sale. The club was incorporated and they made every effort to make people quit thinking it was an inn and to make them quit coming there. From the beginning of the club no meals were served to the public. If a group from Hamburg came up for an evening, they could get meals only if a member was with them or if they had guest-cards. The club is entitled "The Bear Mountain Athletic Club, Inc." on the certificate of incorporation of a membership corporation (the certificate was admitted in evidence). It is the duty of the clerk, Howard, to see that everyone who enjoys the club and equipment has a membership or guest card. Howard does his duty well. Members can bring guests if they want to, but they are supposed to sign for them. There are definite rules against the taking of members' property for safekeeping. There are lockers in the club which members rent, but it is definitely understood by all that the club assumes no responsibility for the contents. The club issues receipts for the payment of dues and of small service bills. (Hummel identified one of the blank forms for receipts.)

The committee thought it advisable to change the signs so that people would realize that it was no longer an inn. A rustic sign was erected across the archway reading "Bear Mountain Club" and a little below it was placed a smaller sign which read "Private Club, For Members Only." They put a sign over the desk which said, "The facilities of this club are open only to persons holding membership or guest cards." This sign could easily be seen from the position where Mr. Meeker testified he was standing, as it is about a foot square. A person driving up the driveway in the afternoon would have the sun behind his shoulder. There must have been a big crowd on June 4. The last hole of the golf course is not far from the clubhouse and he thinks over to the right.

Frank Fleharty is a good friend of Hummel and the witness has several times taken him to the club. Mr. Fleharty is able to go to the club by himself because he is an old friend of Hummel; he likes fishing and Hummel, knowing he was planning to come back after the inn became a club, told Howard and Wheeler to put him up and not ask any embarrassing questions or make him feel that he was not wanted. If he was not Hummel's guest or the guest of someone else in the club, they would stop him and ask for credentials and a card. If he did not have them they could not let him in.

XIV. *Samuel Hummel. Third witness for the defendant (cross-examination).* Witness heard Wheeler testify that the picture which is in evidence was taken by the club, but he does not believe it was taken by the club; it was taken by a member for the purpose of persuading the trustees to have the sign changed to its pres-

ent form. That was between four and five years ago. The only other picture witness has of the entrance is an airplane view. Frank Fleharty was a good friend of his but he had to pay the regular service fees that a member's guest would pay, which is more than a member would pay. To the knowledge of witness, Fleharty should not have anything against the club. He cannot explain Fleharty's testimony that others, who were neither members nor guests, were allowed to go into the club, and that he knew of only one case where they were turned away. He thinks the statement erroneous, but Fleharty must have had some basis for it.⁴

XV. *Philip Meeker. Rebuttal.* Witness took the photograph of the club entrance which was introduced in evidence earlier in the trial. He took it about 6:15 P.M., June 4, as he was leaving the club. The manager had told him there was a sign which he should have seen, so he took particular notice on the way out and decided it would be a good idea to have a picture of it. He had the picture enlarged a short time after he returned from the trip—probably within the week. He saw the original print, which is the same as Exhibit C. The picture is of the left side of the entrance. There is another post on the right which is not shown in the picture. Witness did not take picture of the right hand post because he was only interested in the two signs. He did not notice any signs on the brick post at the right, but he did not look carefully because the manager had said the two signs were both on the left post. If there had been a rustic sign two feet high spread from gate post to gate post, he would have seen it, but there was no such sign.

Mr. Meeker is a jeweler, having been in the business about 20 years. About 12 years ago he gave a diamond bracelet and platinum brooch to his wife for Christmas. He got it wholesale and paid \$600 for the brooch and \$500 for the bracelet. Before he left home on this trip he gave his wife \$450 and he watched her count out \$400 to the clerk at the inn.

When he was first on the stand, he did not testify that he was a jeweler because he was not asked. The plaintiff in this case is the wife of the witness, and he wants her to win.

XVI. *Summing up by counsel for defendant.* A review of the plaintiff's story reveals a peculiar amount of coincidence. Mrs. Meeker, on her trip, stopped for two nights at hotels. On neither occasion did she check her valuables. She says that she slept with \$1500 under her pillow. She and her husband had forgotten where the "club" was, so they asked a native of Hamburg who told them the way to "the Inn." Although they did not remember how to get to the place, they remembered

⁴ We omit from our paraphrase of the stenographic report evidence which showed that the witness was a German, that thrice during the World War he had been arrested for sabotage, that the incorporators of the Bear Mountain Club were members of the German American Bund, etc. The judge in his instructions warned the jury that this testimony was to be wholly disregarded, that it had no bearing whatever on the issues of the case. This evidence was introduced for the purpose of measuring the effect of a political prejudice on the making of a verdict. Jury C, a control jury, was twice sent out of the court room, and heard neither the testimony nor the instructions of the judge. Every member of the other juries had previously shown, by means of a carefully prepared inventory, some degree of prejudice against Nazi organizations in America. The results were negative. Only one juror even mentioned this evidence in the jury room and he was at once reminded of the judge's instruction. Jury A found for the defendant, and every vote in Jury B against the defendant has been accounted for. There is no evidence that the prejudice has any effect whatever.

the location of the room they had 20 years ago. The fact that the people of Hamburg still spoke of the Club as "the Inn" is of no significance, since they had been accustomed to speak of it in this manner for many years before the change in ownership took place. The Meekers, following directions, turned into the driveway, with the sun at their backs, and yet they did not see the two signs over the roadway. Remember, these were stretching from post to post and were new. When they parked the car, no hellboy came to help with luggage. Although a big golf tournament was in progress and the 18th hole was near the driveway, they saw no crowd. No doorman met them as they went into the Club. Passing through the lobby, they failed to see the sign over the desk. There was no clerk, or anyone else in the lobby. Yet the Meekers said they still had no suspicion that this was not a hotel. Though she had slept with her valuables under her pillow at the two previous stops, Mrs. Meeker immediately turned her money and jewelry over to this stranger who suddenly appeared behind the desk. What has become of this mysterious Mr. X, this clerk of whom no one except the Meekers has seen or heard? Note the receipt which he is supposed to have given the plaintiff in return for her valuables. On its back is the printed title, "Cornell Law Library." Mrs. Meeker said she read the receipt carefully, yet she failed to pay attention to the words written under the signature: "Bear Mountain Club." The Meekers went walking for two hours—past the ski jump, past the golf course, past the tennis courts; still they did not suspect that the place was changed.

In order to establish liability, there must be a host-guest relationship. There was no such relationship here. The plaintiff did not even know if she would be able to get a room.

The Club was not negligent. It had a definite, functioning system to keep non-members and guests out. The testimony of Fleharty was rebutted; he got in because he was the guest of the trustee, Hummel. The clerk's absence from the desk was not negligence; he was attending to the duties of his job. The Club had put up signs, it had erected a ski jump, and it had built a golf course and tennis courts. It had done all that could reasonably be expected. Surely it was not necessary that the building be torn down in order to inform the public that it was no longer a hotel.

In regard to the value of the property which the plaintiff claims to have lost, the only indication is the Cornell Law Library slip signed by an unknown imposter who conveniently disappeared. Mr. Meeker did not think of his story of the value of the jewels until he had completed his testimony and the trial was practically over. To intelligent men and women it must be apparent that the plaintiff is attempting to perpetrate a hoax. The jury must find for the defendant.

XVII. Summing up by counsel for plaintiff. If you look at the history of the law pertinent to this case, you find that an innkeeper is considered a member of a common calling, and that he therefore has exceptional duties to the public. Innkeepers often resort to subterfuges of one sort or another in an attempt to escape these duties. But the law looks to the fundamental facts and not to the window dressing. What is the test of an innkeeper? Commonsense. You must disregard names and ask, "Is he doing an innkeeper's business?"

This place had all the appearances of an inn. There was nothing to indicate that it was not an inn. One who had been there before would find no outward changes sufficient to make him suspect that it was no longer an inn. On the contrary, the

appearance was such that any member of the public might reasonably have thought this was what the plaintiff thought it was, a hotel or inn.

Is there any reason to doubt the plaintiff's credibility? It is evident that her story was straightforward and honest, and that her acts were the acts of any reasonable person. Is Fleharty dishonest? There we have the testimony of a completely disinterested party to the facts that this place is operated as an inn. We have the picture of the sign which shows that it was small, half-covered, and completely inconspicuous. By leaving the desk accessible to outsiders, and by not having some one on duty, the inn allowed agency to be established between itself and the man who took the plaintiff's valuables. The reason Mrs. Meeker kept her jewels with her on previous occasions was that those hotels were formal, whereas she knew the inn to have an air of informality and that her jewels would be unnecessary there.

Defendant has attempted to make much of the fact that the Meekers did not see the crowd while the golf match was going on. But note that there has been no testimony as to where the golf match was. Obviously the crowd was not on the 18th green at 4 o'clock when they arrived and still there at 6 o'clock when they came back in.

The jury should find for the plaintiff.

XVIII. *Judge's instructions to the jury.* The major question presented for the consideration of the jury is: "Did the defendant, Bear Mountain Club, by its conduct, reasonably lead the plaintiff to believe that it was offering the accommodations customarily provided by an inn or hotel?" Incidental to that is the question: "Did the plaintiff reasonably rely upon this representation?"

The burden is upon the plaintiff to establish the truth of her statements by a preponderance of the evidence.

An innkeeper has the duty to receive the valuables of the guest and to keep them safely. An establishment may operate as an inn and incur the liabilities of an inn even though under its corporate powers it was not authorized to engage in such occupation. You must decide whether the Bear Mountain Club was actually doing business as an inn, or whether it held itself out to be an inn in such a manner as reasonably to induce Mrs. Meeker to rely upon its having such an apparent character.

If you find that the Club was actually or ostensibly a hotel, you must decide whether the relation of host-and-guest was ever established. Did Mrs. Meeker come to the Club seeking the hospitality which hotels commonly offer, and did the Club begin to furnish such services? To decide this you must determine whether the Club should be estopped from denying the authority of the supposed clerk to receive the valuables. Did the Club permit this stranger to appear behind its desk and to seem to be its employee in such a way as reasonably to mislead a prospective guest?

"If then you find that the Bear Mountain Athletic Club was actually or ostensibly an inn, if you find that Mrs. Meeker became a guest, and if you find that the ostensible clerk acted with apparent authority, you will find for the plaintiff; otherwise you will find for the defendant."

RESULTS

In the court room. The results fall into two groups: those obtained in the courtroom, and those found in the jury room. The numerical results

showing the distribution of the jurors' judgments in the court room on the 9-point scale for each of the installments are shown in Table I. A graph showing the mass tendencies of their judgments for each of the installments is shown in Fig. 1. We have taken the median value as representing the central tendency, and the quartile deviation on each side of the median as representing the variation. The heavy central line in the figure represents the course of the median value of the jurors' judgments; the width of the figure shows the quartile deviation above and below the median. A summary statement of the results shown in table and figure at each stage of the trial follows.

I. Plaintiff's opening statement resulted in a slight belief that the defendant was liable. Six of the 41 jurors had a strong belief that he was

TABLE I
THE DISTRIBUTION OF JUDGMENTS ON THE 9-POINT SCALE

Scale values	Stages of the trial																	
	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV	XV	XVI	XVII	XVIII
9																		
8	6	1	1		1	2	4	6	2	3							1	1
7	6	2	12	3	5	5	6	9	5	4	3	3	3	3	6	1	4	5
6	7	3	14	4	11	6	14	10	5	3	3	6	6	3	3		3	3
5	16	22	5	9	16	14	9	12	10	13	7	6	4	4	11	7	8	9
4	3	4	5	14	2	9	6	3	8	5	14	13	7	9	7	3	3	2
3	2	7	1	7	2	1	1	3	8	10	6	7	16	5	5	4	9	7
2	1	2	1	3	3	3	1	1	1	1	1	1	6	3	2	10	8	5
1	1	1	1	3	1	1		2	2	3	2	5	2		7	4	7	
Median	5.9	5.3	5.4	4.6	5.7	5.4	6.2	6.4	5.1	5.1	4.4	4.5	3.5	4.5	5.3	2.7	3.0	4.7
Q ₁	5.2	4.3	5.2	3.9	5.1	4.5	5.2	5.5	3.9	3.7	3.3	3.6	2.3	3.1	4.2	2.1	2.7	2.6
Q ₃	7.2	5.7	7.2	5.5	6.5	6.4	6.9	7.5	6.3	5.9	5.3	5.7	4.9	5.8	6.6	4.2	5.8	6.2

liable, 16 expressed doubt, and 6 felt some degree of belief that the defendant was not liable. Of the jurors, 19 had, however, some belief in the liability of the defendant.

II. Defendant's opening statement brought the median down toward doubt. Only 6 jurors had any belief in the liability of defendant; 22 expressed doubt; and 13 had some belief in his non-liability. Thus it appears that the opening statements of the attorneys left the jurors relatively open-minded.

III. The direct examination of the plaintiff brought the median to the highest point in the plaintiff's favor that was reached throughout the trial—a slight to fair belief in liability of defendant. The judgments of 26 jurors fell in this category, with one convinced of liability. Only 5 remained doubtful, with 9 jurors holding to some belief of non-liability.

IV. Defendant's cross-examination of the plaintiff was very effective, not only neutralizing the effect of the plaintiff's testimony but also

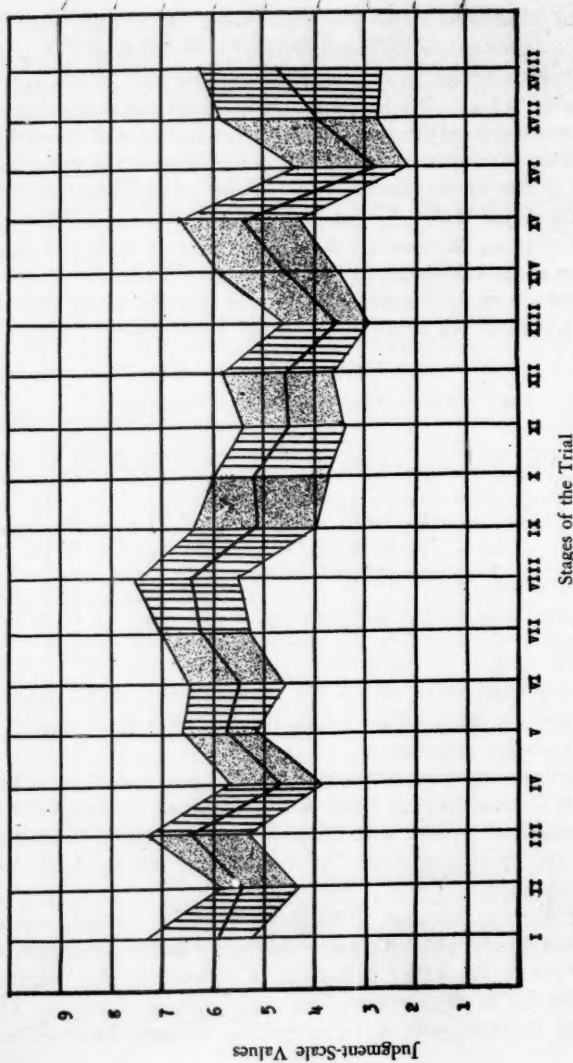


FIG. 1. THE COURSE OF THE JUDGMENTS AT EVERY STAGE OF THE TRIAL
 The central black line represents the median value of all the judgments for each stage. The width of the figure represents the quartile deviation above and below the median. The hatched portion shows the effect of the evidence for the defendant; the stippled portion that for the plaintiff.

bringing the median judgments down below the middle rating of doubt to slight belief in non-liability. Of the 41 jurors, 25 expressed doubt and 3 were convinced that the defendant was not liable. (C)

V. Plaintiff's second witness brought the median back to almost the same position that was reached after plaintiff's opening—16 jurors were doubtful, and 17 had some degree of belief in the defendant's liability.

VI. Defendant's cross-examination of plaintiff's second witness had only a slight effect, moving downward in the direction of doubt. (K)

VII. Direct examination of plaintiff's third witness moved the median above slight belief in liability, with 4 jurors holding a strong belief in liability, and 20 with a slight to fair belief in liability. Only 8 had any belief in non-liability.

VIII. Defendant's cross-examination of the plaintiff's third witness proved harmful to the defendant. The trend was toward liability, with only 4 jurors showing any degree of belief that the defendant was not liable. This is the second highest point in the plaintiff's favor, the median being only 0.01 below its position following plaintiff's first witness (Stage III). (E)

IX. Defendant's first witness was effective, bringing the median down a whole step, to doubt.

X. Plaintiff's cross-examination of defendant's first witness had little effect, moving the median up only 0.04. (F)

XI. Direct examination of the second witness for the defendant resulted in 28 ratings of some belief in non-liability. Only 6 at this point had some degree of belief in liability.

XII. Again cross-examination by plaintiff had little effect, moving the median up only 0.10. (G)

XIII. Direct examination of the defendant's third witness brought a new low, the median going to slight to fair belief in the defendant's case. Three jurors had fair belief in liability, and 4 were doubtful. All the rest had some degree of belief that the defendant was not liable, with 5 convinced of defendant's non-liability and 6 with a strong belief in non-liability.

XIV. Cross-examination of the third witness moved the median up almost a complete step, thus restoring the position held just before the third witness took the stand. (plaintiff's husband)

XV. The rebuttal testimony of Mr. Meeker raised the median another step in plaintiff's favor, to a bit above doubt.

XVI. The summing up by the defendant's counsel was the most effective stage of the entire trial. The median was brought down to between

fair and strong belief in non-liability, and only one juror expressed any belief whatsoever in liability. Seven were convinced of non-liability, and 19 had strong belief.

XVII. Plaintiff's summing up, though effective, did not restore the median to the position held at the conclusion of the testimony. It brought the median up to slight belief in non-liability.

XVIII. The judge's charge to the jury proved helpful to the plaintiff's case, raising the median almost a step but leaving it still in the defendant's favor. The ratings had greater scatter at this point than at any other stage. At this final stage only 11 jurors had any degree of belief in liability, 9 were doubtful, and 21 had some belief that the defendant is not liable. Of the latter, 7 were convinced and 5 had a strong belief in non-liability.

In the jury room. None of the juries reached a unanimous decision on the first poll, so that three more polls were taken. Only one juror out of the 41 changed his vote on subsequent polls. That juror, in Jury A, voted for the plaintiff on the first poll and for the defendant thereafter. Thus the final vote at the end of the 30-min. period was as follows: Jury A (men), 10 to 2 in favor of the defendant; * Jury B (women), 8 to 4 in favor of the defendant; Jury C (mixed), 11 to 6 in favor of the defendant.

In the jury room all votes, with four exceptions, were forecast by the final judgments in the court room. Of the exceptions three had expressed doubt in their final rating but on every poll in the jury room voted for the plaintiff. These three explained that despite their expression of doubt they had had a slight leaning toward the plaintiff. The other exception was the one juror who changed his vote as a result of the discussion. He explained that this was because he accepted the contention of the other jurors that the testimony of the Meekers was unsubstantiated by other proof.

It should be realized that the judgments in the courtroom were not based on "the preponderance of the evidence." Consequently, a juror who had indicated a judgment of only slight or fair belief in liability could still say there was a preponderance of evidence to establish the defendant's liability. Likewise, a juror who left the courtroom with a judgment of doubt might in the jury room contend that there was no preponderance of the evidence for the plaintiff. In point of fact, there were 5 jurors who voted for the defendant on this basis.

⁶ The vote of Jury A is in New York State sufficient for a decision in a civil case. See 463 A of the Code of Civil Procedure.

Individual differences. Thus far our study of the results has been limited solely to the trends of the mass judgments. The quartile deviation has, it is true, given some idea of the relative distribution and the tables have shown the actual distribution of the judgments. We have, however, not questioned how any individual member of our jury reached his particular judgment. The only way in which anything like complete knowledge of this question could be reached would be either by questioning every member or by asking him to write a commentary on his judgment at the time every judgment was made. Neither could have been done without interfering with the progress of the trial. We can, however, study the complete report of an individual and by judicious questions after the trial get some insight into the basis of his ratings and votes. We have done this in some instances. Whenever we found a report that differed markedly from the others it was put aside for further study. Our findings for some of these cases are informative.]

Five of the jurors seldom, if ever, made a judgment below 5. Most of their judgments were either 7 or 8, thus showing a fair or strong belief that the defendant was liable for damages. These jurors formed a slight belief in liability after the opening statement of the plaintiff. For one juror this judgment was strengthened by the opening statement of the defendant because, as he said, "If the defendant should prove what he claimed he would prove he would still be liable." For this group of jurors, all subsequent evidence, the summing up, and the judge's instructions only slightly increased or decreased the certainty of their original judgment, and in the jury room all five voted consistently for the plaintiff.

Four jurors were with the opening statement of the plaintiff left with a slight to fair belief that the defendant was *not* liable; thereafter they never at any time felt that a preponderance of the evidence was in favor of the plaintiff. In the jury room they consistently voted for the defendant.

In one case a juror who began with a conviction of non-liability maintained her belief in the defendant's case until the summing up by the plaintiff, when she was convinced of liability. Her vote in the jury room was for the plaintiff.

Individuals often react differently to the same testimony. Thus, testimony which some considered helpful to the plaintiff was thought by others to strengthen the defendant's case, and still others thought it to be of no importance one way or the other. In the same manner, the reaction to the personality and behavior of some of the lawyers varied markedly with individual jurors. According to statements of the jurors, judgments were

sometimes affected by admiration for or antagonism toward certain members of counsel.

All the jurors did not reach decisions through a logical analysis of the case; some could not explain or justify their conclusions. Thus several reported that they voted as they did because the party "just didn't have any business recovering in a case like that," or "it just didn't seem right to me." An examination of the reports of the jury foremen reveals, however, that the juries as a whole were well aware of the important issues in the case as set forth by the judge. . . .

[*Sex differences.* No outstanding differences were found between the men and the women jurors. When the medians of the men's judgments were plotted on the same graph with the medians of the women's judgments, the curves were found to coincide in most places and to be quite similar throughout. In the jury room, 13 out of 19 girls, and 16 out of 22 boys voted for the defendant; the ratio is almost exactly the same.]

[*Discussion of results.* We cannot assume of course, that other juries would, in all respects, give the same results as our juries did. We may, we think, dismiss the possibility that our jurors were influenced in some unusual way because the trial was a moot and not a real trial. It was held in a room designed and furnished like a conventional courtroom, and the proceedings were conducted with the decorum and seriousness of an actual trial. There are, however, two factors which might have distinguished the work of our juries from that of other juries. In the first place our jurors were probably superior in intelligence and education to the average juror. In the second place, the fact that the case was in part fabricated and the small number of witnesses may have simplified the task.]

[On the other hand, when we consider the psychological processes involved in reaching a verdict, certain of our results which agree with the results of other experiments, may be regarded as typical of the average juror.] We have substantiated the finding of Weld and Roff that "the direction of the mass tendency as represented by the median is in general a function of the testimony,"⁸ i.e. the judgments of the jurors tend to support the contention of the plaintiff or of the defendant as the testimony favors the one or the other side. The first witness for a side is usually the most effective; and although we found that subsequent witnesses were usually less we did not find with Weld and Roff that they tended to become progressively less effective. It is probable that our number of witnesses was too small to show this result, and it is possible that the cross examination interfered with the trend.

⁸ *Op. cit.* 622.

Our results show, however, that the oscillation of the judgments during the course of the trial is influenced not only by the testimony but also both by the opening and closing statements of counsel and by the personality of the witnesses or of members of counsel. As instance of the former, the opening statement of the plaintiff set up a belief in the liability of the defendant exactly as the testimony of a witness for the plaintiff would have done. Similarly when the defendant summed up his case he did more for it than any one of his witnesses had done; and he did it in part by laying less stress upon his proof and more upon the fact that the Meekers had no proof that they had in fact deposited valuables at the desk, thus casting suspicion on the honesty of the Meekers—a bit of strategy for which the plaintiff apparently had no defense.

As regards the influence of personalities, from the first some jurors were convinced of the sincerity and honesty of the Meekers, whereas others mistrusted them; the men were slightly more influenced by Mrs. Meeker's testimony than were the women jurors; the attorney for the plaintiff who made the opening statement was by voice and manner more persuasive than was his colleague who did the summing up.

We also found, as Weld and Roff found that early in the trial many jurors reached a fairly definite decision, and that thereafter the effect of the testimony was merely to change their certainty. This happened with at least 25% of our jurors, half of whom were disposed in the one, and half in the other way. Even those who as the trial progressed developed an attitude of doubt were still inclined to favor the one side or the other and in the jury room did so. We found no juror who attempted to maintain an attitude of doubt on the theory that he should make no decision until he had heard all the evidence, but we can substantiate the earlier findings of the cautious juror, and of the juror who is easily swayed by the testimony. In addition we discovered jurors who were influenced in reaching their verdict less by the proof than by their notion of what under the circumstances seemed to them to be "right" or just.

The fact that our results show no significant sex differences need occasion no surprise. They agree with Burt⁷ as against Münsterberg that women are no more tenacious than men in maintaining their opinions during discussion. Our results from the jury room do not agree with those of several experiments made on the formation of opinion by discussion. As we have shown, only one of our 41 jurors changed his vote in the jury room whereas

⁷ H. E. Burt, *Sex-differences in the effect of discussion*, *J. Exper. Psychol.*, 3, 1920, 390-395; *Legal Psychology*, 1931, 159 f.; H. Münsterberg, *Psychology of Social Sanity*, 1914, 181-202.

in most if not all experiments on group opinion changes in opinion resulting from discussion are frequent.⁸ The reason for our result is probably due to the fact that our jurors had already reached a decision when they came to the jury room and they were prepared to defend their opinion. Our results also give positive evidence in support of those advocates who question the value of cross-examination except for special reasons.⁹

Finally we agree with Weld and Roff as regards the general nature of the thinking process in reaching a trial-verdict. In addition we have discovered the existence of personal standards of evaluation which the juror brings to the trial (p. 533), and we also have evidence that if the prestige of a member of counsel is sufficient, some members of a jury may accept his opinion and maintain it as their own. In no other way can we explain the effect on some jurors of the summing up by the defendant counsel...

SUMMARY

- (1) The judgments rose and fell with the evidence.
- (2) The opening and closing statements of the attorneys were important.
- (3) The effect of cross-examination was variable. In some instances it weakened or even nullified, and at one point (Stage VIII) it strengthened, the effect of the testimony given in the direct examination.
- (4) No significant sex differences were revealed.
- (5) Some opinions were formed on the basis of the opening statements. In some cases a decision was reached early in the trial and all subsequent evidence was interpreted in the light of that decision. In others, decisions were not reached until the end of the trial. The weight given to any particular testimony was influenced by all the preceding judgments, and particularly, by the immediately preceding judgment.
- (6) The juror reached a decision before going into the jury room and the arguments of his fellow jurors who did not agree with him were not effective in changing his decision.

⁸ For references see Weld and Roff, *op. cit.*, 610.

⁹ See J. H. Wigmore, *Science of Judicial Proof*, 1937, 279 ff.

THE APPARENT SIZE OF THE MOON AS A FUNCTION OF THE ANGLE OF REGARD: FURTHER EXPERIMENTS

By ALFRED H. HOLWAY and EDWIN G. BORING, Harvard University

In a previous paper we have shown that the apparent size of the moon diminishes as the angle of regard of the head and eyes is increased above the primary position, if the primary position be here loosely defined as what would be horizontal regard for an erect O .¹ We have shown further that the functions relating apparent size to the angle of regard hold for the supine O as well as the erect, so that the supine O , with head toward the moon, sees the horizon moon as small and the moon in culmination as large. We worked for the most part with direct observation of the full moon, and found the ratios of the greatest to the least linear size to vary from 1.25 to 2.00.

The present paper deals with the apparent size of mirrored images of the full moon (and the sun). Such images are quite as satisfactory for observation and for the production of the illusion as is direct observation, provided the right conditions obtain.²

In the present research we controlled the position of the head by the use of an adjustable biting-board. The need of such control seems obvious enough now that we are sure that the illusion depends upon the movement of the eyes, but it was not obvious at first.

We have used two methods of measurement. (1) The first, described in our earlier paper,³ consisted in equating the size of a circle of light, projected by a lantern on a screen 3.5 m. away, to the size of the full moon. In our previous experiments we placed the projection screen at one side of O , so that, erect or supine, he had to turn his head through 90° to see the comparison stimulus. The criticism was made,⁴ however, that this turning of the head might vitiate the comparison. In the present experiments we placed the screen directly in front of O , omitting observations in the supine position, which loses its importance once one is convinced that the orientation of the body with respect to the earth is no longer to be considered relevant to the illusion. (2) We also introduced two simultaneous images of the moon (or sun) for direct comparison, having O estimate the ratio of the diameter of one to the diameter of the other.

The use of mirrors speeds up observation because with them it is possible to get the moon at any elevation at any time during the evening. On the other hand, there

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¹ A. H. Holway and E. G. Boring, The moon illusion and the angle of regard, this JOURNAL, 53, 1940, 109-116.

² *Op. cit.*, 114 f.

³ *Op. cit.*, 110 f.

⁴ At the Princeton meeting of the Society of Experimental Psychologists, 1939.

is always the delay of waiting for good weather at the full moon. There are only about three consecutive nights when the perceived moon is near enough to a circular disc to be matched to the circular comparison stimulus. One has, on that account, to wait for a full moon on a perfectly clear and not very windy night. There must be no clouds across the face of the moon nor must there be haze in the sky, for in such cases the mirrors become visible and reduce or abolish the illusion. On a hazy night one sees the moon, set in a golden misty square, corresponding to the

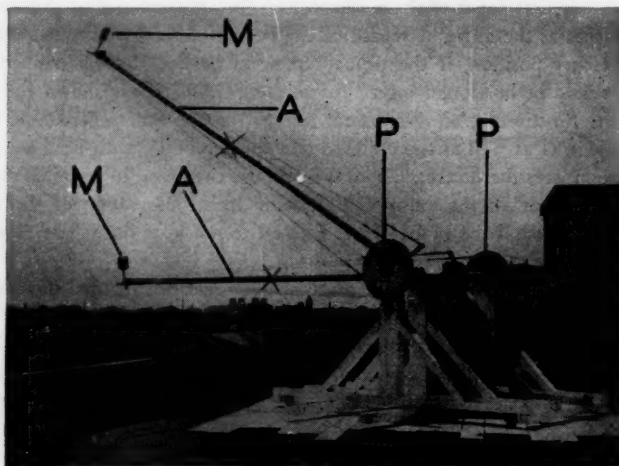


FIG. 1. APPARATUS

MM = mirrors, adjustable about two axes of rotation. AA = mirror-arms, each 5 m. from fulcrum to mirror. PP = protractors for measuring the elevation of the arms.

shape of the mirror, upon the black background of the unilluminated sky. On a windy night the mirrors may shake too much. It would be much more convenient, therefore, to use the sun, since a 'full' sun is to be had oftener than a full moon, especially during normal working hours. We got the illusion by viewing two mirrored images of the sun simultaneously through dark filters, but the method proved less satisfactory than we had hoped.

APPARATUS

Figs. 1 and 2 show two views of the apparatus. The long arms, 5 m. from fulcrum to mirror, were made of two sections of corrugated iron conductor pipe, brazed together and stiffened by steel guy wires. We also had short arms, 2.5 m. long. The mirrors at the ends of the arms were supported by two worm wheels. One wheel rotated the mirror about the axis of the arm, the other about the axis of the mirror support, which was at right angles to the axis of the arm. The worms were attached to spools about which fish-line was twisted, so that the worms could be turned by

pulling on the lines when the mirrors were aloft. The mirrors were front-aluminized, 6 in. square. It was always possible to get the image of the moon simultaneously for both eyes within the square of the mirror, provided the moon itself were in the quadrant of the heavens behind the observer. Since the apparatus was moveable, it could be shifted as the moon southed, but it would have been more convenient to

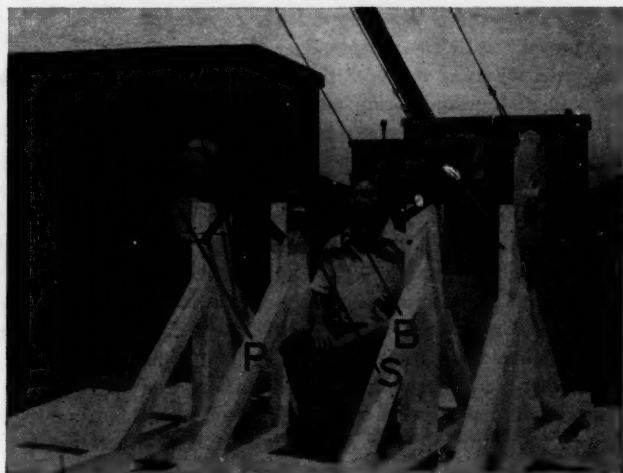


FIG. 2. APPARATUS

PP = protractors for measuring elevation of the arms. B = biting-board, pivoted at the ends, for controlling angle of head. S = stop-plate for biting-board, arranged so that board can be locked at any 15° -angle, or can be freely moved back and forth through an angle which is a multiple of 15° .

have had larger mirrors, so that the worms would not have had to be constantly readjusted as the moon's image moved off the mirror for one eye or the other.

The biting-board was fixed to the apparatus between the axles of the two arms and a few inches below them. It consisted in a long plate of cadmium-plated brass, pivoted at each end so that it could be rotated into different planes. A set of stops was arranged to lock the biting-board in the horizontal position, the vertical position, or any 15° -interval between. The stops could also be arranged to allow the board to rotate freely through some predetermined angle that is a multiple of 15° . The erect *O* sat on a chair and bit the biting-board. The supine *O* lay on a table adjusted so that he too could bite the biting-board.

Other details of procedure are given above or in the previous paper there cited, or below in connection with the discussion of the results.

EYE-MOVEMENT VS. HEAD-MOVEMENT

The eyes can be elevated above the primary position between 40° and 45° . Upward vision through spectacles may be limited to 30° above the

horizontal. If the moon is viewed at 60° -elevation, the neck must bend back at least 20° . To view the actual zenith an erect *O* must raise his eyes 45° and bend his neck 45° , or else, if he finds this impossible, he must also bend his body. Similarly the supine *O*, viewing the horizon moon beyond his head, must bend his neck and body 50° to see the moon with eyes raised 40° , altogether a very uncomfortable posture, which one *O* in our last experiment was unable to maintain without nausea.

It has been suggested that both the elevation of the eyes and the backward tilting of the head, being positive in character, act to shrink the apparent size of the moon,⁵ since both occur in the free viewing of the illusion. They should, therefore, be separately controlled, and we find by such an analysis that the illusion is dependent entirely upon the eye-movement and not at all upon the head-movements.

O, erect, fixed his head by biting firmly upon the biting-board locked in the horizontal position (Fig. 2). Only one arm of the apparatus was used. *O* observed the mirrored image of the moon at 0° (horizon), 5° , 10° , . . . 30° or 35° elevation. He matched the moon for size by a projected comparison stimulus on a screen 3.5 m. in front of him. The screen was placed so that the horizon moon and the comparison appeared side by side. The results appear in Table I and Fig. 3, a smooth function for each of the two *O*s, with the maximal illusion measured by the ratios 1.8 and 2.1. These ratios in themselves, being in each case slightly larger than the largest ratio found for each of these *O*s in the previous experiment, indicate that the entire illusion can be derived from eye-movement alone.

We also arranged the biting-board so that it would rotate freely between stops at 0° (horizontal) and 30° elevation. *O* first matched the moon at 0° to a comparison stimulus, also at 0° . Then he viewed a moon at 30° by bending his head back 30° so that the biting-board was pressed against the 30° -stop, and matched this moon to the comparison stimulus with his head at 0° , bending his neck back and forth through 30° until he was satisfied with the match. Since the angle of elevation of the moon was taken up entirely by movement of the head, the eyes viewed both the moon and the comparison stimulus from the primary position. The result is shown in Table I and Fig. 3. There was no illusion.

It is now plain why the observed points in the previous experiment did not always fall consistently on smooth curves.⁶ The scatter was undoubtedly due to the uncontrolled movements of the head. By keeping

⁵ E. Schur, Mondtäuschung und Sehgrösskonstanz, *Psychol. Forsch.*, 7, 1925, 44-80, esp. 75 f.

⁶ Holway and Boring, *op. cit.*, 110, Fig. 1.

the head rigid we can get smooth, consistent functions, like the curves of Fig. 3.

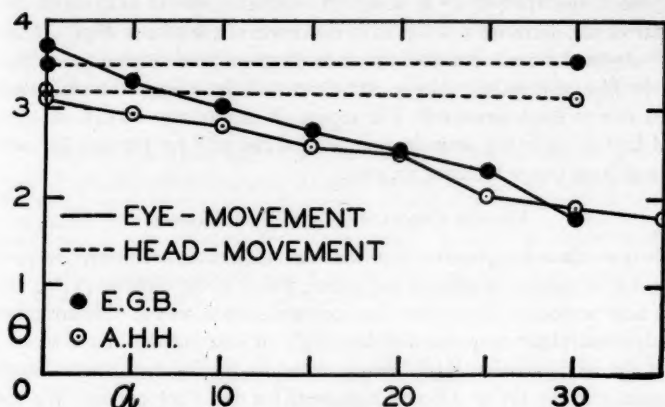


FIG. 3. APPARENT SIZE OF MIRRORED MOON AS A FUNCTION OF EYE-MOVEMENT AND HEAD-MOVEMENT

α = degrees of elevation of eyes above horizontal for erect O . θ = apparent size of moon in degrees subtended. These are the data of Table I, q.v.

TABLE I

APPARENT SIZE OF THE MIRRORED MOON AS A FUNCTION OF EYE-MOVEMENT AND OF HEAD-MOVEMENT

α = angle (degrees) of elevation of the mirrored image of the moon from the horizontal. θ = visual angle (degrees) subtended by comparison stimulus at 3.5 m., equated in size to moon's image. σ = standard deviation of the distribution of θ . Erect posture. For eye-movement: head is fixed by fixed horizontal biting-board. For head-movement: biting-board moves through same angle as moon's image. Each θ is the average of 5 observations. Long 5-m. mirror-arms. Max. ratio = ratio of θ s for maximal and minimal α s. September 1, 1939. See Fig. 3.

α	E.G.B.				A.H.H.			
	Eye-movement		Head-movement		Eye-movement		Head-movement	
	θ	σ	θ	σ	θ	σ	θ	σ
0	3.72	.08	3.50	.08	3.16	.31	3.21	.22
5	3.31	.12			2.95	0		
10	3.02	.17			2.81	.06		
15	2.77	.12			2.59	.10		
20	2.56	.08			2.46	0		
25	2.20	.18			2.01	.11		
30	1.77	.12	3.57	.12	1.90	.09	3.15	.19
35	Too high				1.79	.14		
Max. ratio	2.12		0.98		1.77		1.02	

It is a corollary of this finding that the entire illusion must occur within 40° of elevation. The moon at 60° , as in the last experiment, is

invisible to head fixed to a horizontal biting-board. In fact, the form of the curves in that experiment indicate that the eyes moved for the ascending moon and erect *O* up to about 25° elevation, for in that region the apparent size decreases steadily, as in the curves of the present Fig. 3. After 25° the head-movements would seem to have come in so that the change in the illusion was less. Above 40° almost all the adjustment must have been due to head-movement. The supine *O*s (especially A.H.H. in 1936 and L.M.H. in 1938) seem to have relaxed the neck for the first 30° and then to have begun to lower the eyes.

DIRECT COMPARISON OF TWO MOONS

It is a common experience that 'absolute' judgments are neither so positive nor so accurate as relative judgments, which reveal differences that had not been suspected. Two colors that look alike when seen at different times in different places may contrast shockingly in juxtaposition. So it seemed that the moon illusion might be enhanced by placing two moons simultaneously in the sky at different elevations for direct comparison. We had the necessary apparatus: two arms and two mirrors.

At once, however, a difficulty arises because neither moon can be varied in size to match the other; yet the most accurate measurement depends ultimately upon the judgment of equality. This is a common dilemma. When it is necessary to compare objects of different size, when neither can be altered to match the other and there is no variable comparison stimulus available, the best that can be done is to estimate the size of one object in terms of the other. We had our *O*s (with heads immobilized by the biting-board) judge the diameter of one moon in terms of the other, and we left them free to choose whether they would judge the larger in terms of the smaller, or conversely. The *O*s found these comparisons difficult. Mostly they used the ratios 1.0, 1.1, 1.2, 1.3, . . . 1.8, occasionally intermediate values like 1.15, 1.25. . . .

The results of this method appear in Table II, which shows the diminishing apparent size of the moon in elevation from the primary position (horizontal) to +30°, all for the erect *O*. The maximal ratios for the illusion are, for the three *O*s, 1.6, 1.5, 1.2. For the supine *O* with the primary position vertical it is possible to investigate both the upper (0 to +30°) and the lower (0 to -30°) quadrants. It will be seen from the columns for the supine position that the apparent size of the moon diminishes, not only as the moon 'ascends' from the primary position, but also as it 'descends' from the primary position. *Apparent size is maximal when regard is from the primary position.* So far as we know,

this is the first time that the lower quadrant (the moon below the horizon) has been investigated. The table shows that the amount of the illusion (maximal ratios) in the lower quadrant is of the same order as the amount in the upper quadrant.

This method of direct comparison of two moons is much less satisfactory than the matching of one moon with a variable comparison stimulus,

TABLE II

RATIOS OF APPARENT SIZES OF MIRRORED MOONS AT DIFFERENT ELEVATIONS

Observed ratios of apparent diameters of pairs of mirrored images of the moon at different elevations. α = angle (degrees) of eyes above (+) or below (-) the primary position. 0 = primary position = horizontal for erect *O*, vertical for supine *O*. All ratios are relative to size at primary position = 1.00. The ratios without parentheses at +30 and -30 are products of the ratios for 0:15 and 15:30, whereas the ratios within parentheses are direct check observations 0:30. The two ratios should be the same. Long 5-m. mirror-arms. The "Max. ratios" are the reciprocals of the least ratios above them, separately for the upper and lower quadrants.

July 1, 1939.

α	E.G.B.		A.H.H.		L.M.H.	
	Erect	Supine	Erect	Supine	Erect	Supine
+30	(.62)	(.72)	(.67)	(.67)	(.83)	(.74)
+30	.62	.64	.69	.69	.83	.70
+15	.80	.83	.83	.83	.91	.91
0	1.00	1.00	1.00	1.00	1.00	1.00
-15		.91		.87		.83
-30		.76		.76		.64
-30		(.77)		(.83)		(1.00)
Max. ratio upper quad.	1.61	1.56	1.49	1.49	1.20	1.43
Max. ratio: lower quad.		1.32		1.32		1.56

because it is so much harder to estimate a ratio than to make a judgment of equality. The *O*s were uncertain of their estimates, and the results check (cf. the double values for +30 and for -30 in Table II) only approximately. The method is useful, however, for preliminary investigation, and for observing the illusion with the sun, when the use of filters would render a comparison stimulus invisible.

ILLUSION ABOVE AND BELOW THE HORIZON

To investigate the illusion below the primary position we first employed the estimation of ratios with an erect *O*. For this purpose we were obliged to use the short 2.5 m. mirror-arms, for the long arms could not have been depressed very far below the horizontal without being stopped

by the roof on which the apparatus rested. Even then we could not get the mirrors lower than 25° below horizontal. The short arms are not so good as the long. With them there is a greater chance of the illusion's breaking down, for it is not so easy to make the mirror and its support invisible as it is when the mirror is at a greater distance. An alternative to making the mirror completely invisible is for the *O* to assume the perceptual attitude of looking through the mirror to a far-off distant object. The fact that the three principal *O*s in this experiment had considerable practice in assuming this attitude may account for the fact that the illusion, as finally measured, is not less than had been found with the long arms.

TABLE III

RATIOS OF APPARENT SIZES OF MIRRORED MOONS AT DIFFERENT ELEVATIONS

This table gives data for both upper and lower quadrants for the erect *O* only. Short 2.5-m. mirror-arms were used. Otherwise it is like Table II. See the legend of Table II for explanation of the entries. October 28-29, 1939.

α	E.G.B.		A.H.H.		L.M.H.	
	Oct. 28	Oct. 29	Oct. 28	Oct. 29	Oct. 28	Oct. 29
+30	.68	.58	.39	.47	.43	.61
+20	.80	.72	.50	.57	.54	.72
+10	1.00	.80	.71	.74	1.00	.90
0	1.00	1.00	1.00	1.00	1.00	1.00
-10	.80	.75	.91	.83	.70	1.11
-20	.76	.60	.70	.69	.49	.88
-25	.66		.63		.49	
Max. ratio: upper quad.	1.47	1.72	2.57	2.13	2.33	1.64
Max. ratio: lower quad.	1.51	1.54	1.59	1.41	2.04	1.43

The data of the estimated ratios are given in Table III, in which the figures have the same significance as the data of Table II. The diameter of the moon in the primary position is taken as unity. The extreme ratios are cumulated, that is to say, they are related to size at the primary position by being the products of three successive ratios. The functions show no inversions, for there is in all six instances continuous diminution on both sides of the maximum, which in all cases but one occurs in the primary position. In general, these functions seem to us to be conclusive as to the fact of diminution of the apparent size of the moon both above and below the horizon, but not reliable as indicating the form of the function or the exact amount of the maximal illusion.

For more precise results we, therefore, returned to the method of matching the moon to a comparison stimulus, still of necessity using the

short mirror-arm on the apparatus. These results are shown in Table IV and Fig. 4. The trends are fully significant. Successive differences average about three times their standard deviations. Every maximal difference is

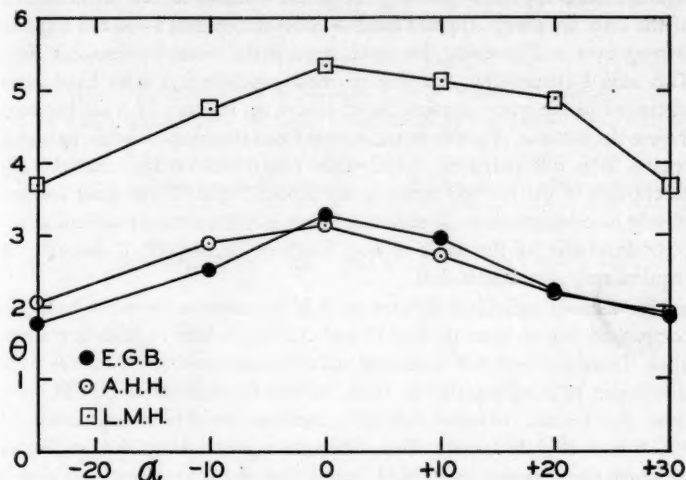


FIG. 4. APPARENT SIZE OF THE MIRRORED MOON ABOVE AND BELOW THE PRIMARY POSITION

α = degrees of elevation or depression of regard above (+) or below (-) the primary position, which is horizontal, being for the erect O . θ = apparent size of moon in degrees subtended. These are the data of Table IV, q.v.

TABLE IV

APPARENT SIZE OF THE MIRRORED MOON ABOVE AND BELOW THE HORIZON

α = angle (degrees) of mirrored image of moon from the primary position (horizontal). θ = visual angle (degrees) subtended by comparison stimulus at 3.5 m., equated in size to moon's image. σ = standard deviation of the distribution of θ . Erect posture, with head fixed by horizontal biting-board. Short 2.5-m. mirror-arms. Each θ is the average of 5 observations. Max. ratio = ratio of θ for 0° to θ s for highest and lowest positions. October 29, 1939. See Fig. 4.

α	E.G.B.		A.H.H.		L.M.H.	
	θ	σ	θ	σ	θ	σ
+30	1.85	.10	1.88	.20	3.66	.21
+20	2.21	.11	2.18	.04	4.88	.22
+10	2.95	.25	2.71	.05	5.16	.16
0	3.26	.25	3.13	.13	5.34	.30
-10	2.51	.11	2.88	.10	4.77	.15
-25	1.77	.14	2.06	.17	3.70	.12
Max. ratio: upper quad.	1.77		1.67		1.46	
Max. ratio: lower quad.	1.84		1.52		1.44	

about five times its standard deviation. There are no inversions. The illusion always appears for a 10° -difference and always in the direction that makes the moon appear larger when nearer the primary position.

The moon, apparently so large upon the horizon to the direct regard of the erect *O*, always appears small—astonishingly small—to the *O* who, leaning over and inverting his head, regards the moon between his legs. This casual observation, familiar to most psychologists who have been interested in the moon illusion, is, of course, an instance of what happens 'below the horizon,' for the moon is seen from this posture with 'lowered' regard. It is our judgment, based upon casual observation, that the apparent size of the horizon moon is not reduced at all if the head is completely inverted, so that the regard is again in the primary position. Complete inversion of the head is not, however, accomplished casually; it requires aids or acrobatic skill.

Fig. 4 shows individual differences. L.M.H. matches the moon to larger comparison stimuli than do E.G.B. and A.H.H., whose matches are about alike. There has been this consistent difference among these three *O*s since they began working together in 1936. Always the matches of L.M.H. have been the largest, whereas A.H.H.'s matches have been equivalent to E.G.B.'s or slightly larger.⁷ This difference is compulsory and unaffected by suggestion. Sometimes, L.M.H. would take the *O*s position and view a match already made by E.G.B. or A.H.H.; then he would exclaim with amused surprise at the great inequality that the others had judged equal. Conversely, L.M.H.'s matches seemed quite impossible to the other *O*s. This difference does not concern us here, because the moon illusion is a matter of relative, not absolute, apparent size for any given *O*. The individual difference is quite another psychological problem, for it pertains to the way in which apparent size varies with distance. For instance, if apparent size depends upon apparent distance, as some have thought, then L.M.H. sees the moon in every position about twice⁸ as far away as do the other two *O*s.

APPARENT SIZE OF THE SUN AS A FUNCTION OF THE ANGLE OF REGARD

We undertook to work with the sun, partly because the sun is more often available than the full moon and partly because we thought that confirmation of the lunar function by a solar function would extend the generality of our findings. We used a heavy black cloth to throw over *O*'s head. In it was inserted a small wooden window-frame, into which could

⁷ Cf. Holway and Boring, *op. cit.*, 110, 112.

⁸ The average ratios of the apparent sizes in Table IV are L.M.H./E.G.B. = 1.93, L.M.H./A.H.H. = 1.88, and A.H.H./E.G.B. = 1.01.

be slid a dense black filter (uniformly exposed photographic film). The window permitted binocular vision, being about 12 cm. long \times 4 cm. high. The filters came inside the point of near vision, being about 3 cm. from the eye-ball; hence they were invisible as objects. The sun appeared through them as a bright disk on a dark field.

TABLE V

RATIOS OF APPARENT SIZES OF MIRRORED SUNS AT DIFFERENT ELEVATIONS

This table gives data for upper and lower quadrants for the sun observed by an erect O. Short 2.5-m. mirror-arms were used. Otherwise it is like Table II for the moon. See the legend of Table II for explanation of the entries. August-September, 1939.

α	Primary position horizontal					Primary position 30° above horizontal				
	E.G.B.		A.H.H.		S.S.	A.B.	E.G.B.		A.H.H.	
	Sept. 12	Sept. 12	Sept. 12	Sept. 12	Sept. 14	Sept. 16	Aug. 24	Sept. 11	Aug. 24	Sept. 11
+35	—	—	.42	.57	.74	—	—	—	—	—
+30	.40	.41	—	.63	—	.57	.61	.60	.62	.63
+25	.57	.59	.50	.63	.87	—	—	—	—	—
+20	—	.84	—	.76	—	.72	.76	.86	.69	.76
+15	.72	.85	.65	.75	.97	—	—	—	—	—
+10	—	1.05	—	.91	—	.90	.90	.95	.83	.91
+5	1.03	.95	.91	.97	1.00	—	—	—	—	—
0	—	1.00	—	1.00	1.00	1.00	1.00	1.00	1.00	1.00
-5	.97	1.05	1.09	1.02	1.00	—	—	—	—	—
-10	—	1.00	—	.87	—	1.11	1.03	.85	.98	.95
-15	.87	.74	.91	.93	.95	—	—	—	—	—
-20	—	.80	—	.79	—	1.23	.82	.76	.89	.76
-25	.74	.74	.83	.77	.85	—	—	—	—	—
-30	—	—	—	—	—	—	.78	.65	.85	.69

Max. ratio:
upper quad. 2.50 2.44 2.38 1.76 1.35 1.76 1.64 1.67 1.61 1.59

Max. ratio:
lower quad. 1.35 1.35 1.20 1.30 1.18 .81 1.28 1.54 1.18 1.45

This method has, however, two drawbacks. In the first place, it necessitates the estimates of ratios, since there is no simple way in which a variable comparison stimulus can be arranged to be visible to the shielded eyes. In the second place, the judgments proved to be more difficult than for the moon. We were inclined to blame this difficulty upon the filters, believing that, although they were invisible in the sense that their microstructure could not be brought into focus by the eye, they nevertheless seemed to fix the distance as too little, in the way that an ordinary visible mirror destroys the illusion for the horizon moon. The experienced Os remarked that with this method the sun looked too small.

The results appear in Table V, which is comparable to Tables II and III for the moon. *The apparent size of the sun, like the apparent size of the moon, is maximal for regard in the primary position, and diminishes when the eyes are raised or lowered.* There are almost no inversions (cf. E.G.B., Sept. 12). The "max. ratios" are about what we found for the moon. There is some indication that the maximal size tends to occur a little below 0° , a displacement that might account for the tendency of the maximal ratios for the upper quadrant to be greater than those for the lower quadrant. The data do not, however, permit of such precise interpretation.

The table shows the functions for two new Os, S.S. and A.B. The function for one of them confirms the general rule, but the other function contradicts it. A.B. estimated the sun's image as larger below the primary position than in the primary position. We checked this difference in several observations and on two different days. It is a fact that can not, of course, be explained so long as there is no theory to explain illusion in general. Both S.S. and A.B. found the observations difficult and unsatisfactory.

The last four columns of Table V confirm our previous finding that the function moves with the head. *O* was tilted back 30° in his chair, and the biting-board was tilted up 30° , so that the primary position was moved to 30° above the horizontal. The data show that the maximum size occurs near the primary position and not near horizontal, which is now at -30° .

The fact that the illusion can be got at all with dark filters which show only the sun's disk on a dark field is one more piece of evidence that the phenomenon does not primarily depend upon the perception of objects on the earth's surface.

Fig. 5 summarizes the chief fact of this investigation. It shows the relative projected sizes of the moon and of the equivalent comparison stimulus for the horizon moon and for the moon at the 30° -elevation. These diagrams presumably represent the spatial patterns on the retina, since the perceptual determinants here under consideration lie almost certainly in the central nervous system. The relations of Fig. 5 approximate the findings for E.G.B. and A.H.H.: moon's diameter = 1, equivalent comparison diameter for moon on the horizon = 7, equivalent comparison diameter for moon at 30° -elevation = 4, height of moon above the horizon at 30° -elevation = 32. (The comparison stimuli for L.M.H. were considerably larger.) The figure makes it clear that two 'illusions' are being measured. (1) The illusion of size due to difference in distance ("size constancy") is the ratio of *C* to *M* in *A*, 7:1. (2) The illusion of size due to elevation of the eyes ("moon illusion") is the ratio of *C* in *A* to *C*

in *B*, 7:4. In *B* the two principles work against each other, so that the ratio 4:1 in *B* is really the product of 7:1 (the 'size-distance' phenomenon) by 4:7 (the 'elevation-distance' phenomenon).

THEORY

The moon illusion is not an *external phenomenon* for its determinants lie within the perceiving organism. The astronomer's plates do not show the illusion.

B. 30° M.

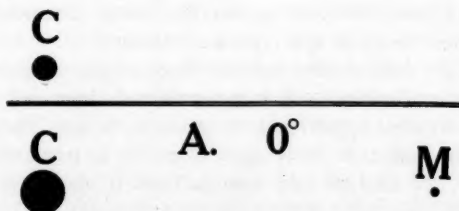


FIG. 5. PROJECTION DIAGRAMS OF THE MOON ILLUSION

M = moon. *C* = comparison stimulus, projected on screen 3.5 m. from *O* and equated in apparent size to *M*. *A* shows relative sizes of *M* and *C*, presumably as projected on the retina, with *O* erect and with *M* on the horizon. *B* is like *A*, except that *M* is elevated 30° above the horizon. The amount of the illusion is shown by the relative sizes of *C* in *A* and in *B*. These relations represent the data for E.G.B. and A.H.H.; for L.M.H. the *C*s would be even larger.

The *object-contrast theory* is the one most commonly held: the moon, it is said, looks large at the horizon because it contrasts with visible terrestrial objects. If there were small objects on the earth and large objects in the heavens, there would be meaning to this explanation; yet there are no objects at all in the heavens except the stars which are smaller than most important terrestrial objects seen on the horizon.

The case against both the external physical theory and the contrast theory is as follows. (a) The horizon moon, seen by an erect *O* is large, but (b) seen between the legs with partially inverted head, it is small, although terrestrial objects remain visible. This change can not be due to the objects' being seen upside down, because (c), if the head is completely inverted by some contortionistic aid, the moon again becomes as large as for the erect head, although the objects are still seen upside down. Another argument is that the large horizon moon (d) becomes small if viewed in an ordinary visible mirror which is near the *O*, but (e) becomes large again if the mirror is made invisible, even though the visible mirror reflects terrestrial objects as well as the moon. It is true, moreover, that (f) the horizon sun or moon may appear large through a mist that obscures the sight of objects, and that (g) in our experiments the illusion occurs for the sun's disk seen through a dark filter which obscures the sight of all other objects.

Another classical explanation is the *apparent-distance theory*. It is supposed that the arch of the heavens appears ellipsoidal so that the zenith seems nearer than the horizon, and the moon, subtending a constant angle but appearing near in elevation, seems smaller. In so far as this theory depends upon the belief that apparent distance to the horizon is enhanced because it is filled with visible terrestrial objects, the argument of the preceding paragraph tells against the theory. The relation of the theory to movements of the eyes is discussed below.

There remain four modern theories which require consideration. The convergence theory and the torsion theory are both dependent on binocular vision and both relate apparent size to apparent distance. The illumination theory and the inhibition theory apply as readily to monocular as to binocular vision. No final decision amongst them is possible at the present time.

(1) *Convergence theory*. Since the raised eyes tend to diverge (exophoria) as the result of muscular imbalance, the innervation for convergence is necessary if the binocular impressions of seen objects are to fuse and diplopia is to be avoided. For the lowered eyes there is increased convergence (esophoria) with diplopia avoided by innervation for lessened convergence.⁹ Zoth's theory of the moon illusion is that this compensatory convergence for the raised eyes causes the moon to be seen as nearer and

⁹ F. B. Hofmann, *Die Lehre vom Raumsinn*, 1920, in *Handbuch der gesamte Augenheilkunde*, 2 ed., III, 1925, chap. 13. See sect. 4 on eye-movements, 259-351, esp. 279-289, 312-320; also Zoth, *loc. cit. infra*.

thus, since it still subtends the same visual angle, as smaller.¹⁰ It is a well-established fact that convergence of the eyes makes near objects appear nearer when their angular size remains unaltered.¹¹ The theory encounters three difficulties. (a) The moon illusion is seen by monocular persons, for whom compensatory convergence to avoid diplopia seems improbable. (b) Observers all agree that the small moon in elevation looks further away than the large moon on the horizon. (c) Our observations contradict the theory, because esophoria below the primary position should make the moon appear larger still, whereas we find its apparent size diminished in the lower quadrant.

(2) *Torsion theory.* When the eyes are raised, the right eye tends to rotate clockwise (as seen from behind) with respect to the left eye. Such torsion of the eyes means that the fields of vision, as seen by the subject himself, also rotate, the field of the right eye counterclockwise with respect to the field of the left eye. The resultant angular displacement is called excyclotorsion, and a little consideration will show that, according to the elementary principles of stereoscopy, it would tend to bring the top of the field of vision near to O as compared with the bottom of the field.¹² This is the condition that the moon illusion is said to require, that the elevated moon should seem nearer to O without loss of angular size. For lowered regard the relations are reversed. There is incyclotorsion which makes the bottom of the field of vision seem nearer than the top, and thus the moon below the horizon should appear smaller than on the horizon. This theory, which is due to Adelbert Ames of the Dartmouth Eye Institute, has, therefore, an advantage over the convergence theory in that it requires the reversal of the direction of change in apparent size at the primary position.

¹⁰ O. Zoth, Ueber den Einfluss der Blickrichtung auf die scheinbare Grösse der Gestirne und die scheinbare Form des Himmelgewölbes, *Arch. f. d. ges. Physiol.*, 78, 1899, 363-401, esp. 393-400.

¹¹ H. Meyer, Ueber die Schätzung der Grösse und der Entfernung der Gesichtsbjekte aus der Konvergenz der Augenaxen, *Ann. d. Phys. u. Chem.*, 85, 1852, 198-207, showed that the convergence of the eyes to fuse two adjacent identical wall-paper patterns makes the pattern appear nearer and smaller. Any repetitive pattern can be used, like a cane chair-seat, a typewriter key-board, postage stamps; see E. C. Sanford, *A Course in Experimental Psychology*, 1898, 273; E. B. Titchener, *Experimental Psychology*, I, ii, 1901, 262; R. S. Woodworth, *Experimental Psychology*, 1938, 676. A recent study is H. Frank, Ueber den Einfluss inadäquater Konvergenz und Akkommodation auf die Sehgrösse, *Psychol. Forsch.*, 13, 1930, 135-144.

¹² See E. E. Maddox, *Tests and Studies of Ocular Muscles*, 1898, 392-395, (3 ed., 1907, which we have not seen). Maddox describes the Meissner test in which, by a reversal of this principle, the cyclotorsion of the eyes is measured by noting how much a fine line has to be tilted from the actual vertical in order to appear vertical. See also Hofmann, *op. cit.*, 276, and esp. L. Howe, *The Muscles of the Eye*, 1907, I, 348-365.

It is known that these size-distance effects from cyclotorsion occur only when they are free of other spatial cues. Perspective motives ordinarily overcome them completely. In the same way the moon illusion occurs only at considerable distances where perspective and other cues to distance are lacking. The consistency between these two sets of facts, therefore, increases the plausibility of the torsion theory. On the other hand, the monocular illusion makes trouble for this theory. The *Raumlage* for the raised or lowered eyes might persist for a time after one eye is removed from function, but it seems hardly likely that a differentiation, that was originally dependent upon binocular disparity, could persist indefinitely in a person who has permanently lost the sight of one eye. Against this theory too is the fact that the moon in elevation does not look nearer than the horizon moon.

(3) *Illumination theory.* We have shown elsewhere that apparent size is diminished when retinal illumination is diminished.¹³ If raising or lowering the eyes reduces the retinal illumination, we might find ourselves in possession of an explanation of the illusion—an explanation which might apply as well to monocular as binocular vision.

It is true that, when the eyes are elevated to an extreme position (ca. 40°), there is a diminution of illumination which may be as small as one-twentieth of the illumination in the primary position. The pupil, as we found by observation through a long-focus microscope, has in this position only 40% of its area left uncovered by the lid. A Macbeth illuminometer indicated that the shading by the lids, lashes, and brows reduced the illumination of the remaining part to 30% of its original value. Stiles and Crawford¹⁴ have shown that light entering the periphery of the pupil is less effective than light at the center, "perhaps due to the angle at which light strikes the retina." The reduction here would be to 40%. Now $0.40 \times 0.30 \times 0.40 =$ about 0.05. With artificial moons at approximately 100 and 200 ft. and with a reduction of illumination to one-twentieth, we found apparent size on the average reduced to a diameter about 90% of the original diameter.¹⁵ This is too small a reduction in size to account for the moon illusion, in which the apparent diameter may be reduced to 90% by raising or lowering the eyes only 10° or to 50% by raising them

¹³ Holway and Boring, The dependence of apparent visual size upon illumination, this JOURNAL, 53, 1940, 587-589.

¹⁴ W. S. Stiles and B. H. Crawford, The luminous efficiency of rays entering the eye pupil at different points, *Proc. Roy. Soc., B*, 112, 1933, 428-450.

¹⁵ Holway and Boring, *loc. cit.* (The dependence of apparent visual size upon illumination).

30°. A more exact investigation than ours, however, is needed to evaluate the effect of intensity.

(4) *Inhibition theory.* Schur has suggested that the effort of raising the eyes or head may be the essential condition for the loss in size of the perceived moon.¹⁰ It is clear that head movements are irrelevant, but it remains possible that raised or lowered eyes may cause the illusion simply because such eye-movement requires more effort than lateral motion. This theory has the advantage over the convergence and torsion theories in that it is applicable to monocular vision, but it is somewhat vague in that it does not suggest how such a dynamic relation between muscular effort and perceptual size could be mediated by the nervous system.

At the moment the monocular theories seem to us more promising than the binocular. What we should like to find is some means whereby eye-movement above and below the primary position diminishes the total excitation of the visual perceptual field.

SUMMARY

(1) The moon illusion does not depend to any great extent upon physical conditions outside the body of *O*.

(2) It does not depend upon the posture of the body or head.

(3) It does depend in its full amount upon the position of the eyes within the head.

(4) The apparent size of the moon is maximal when the eyes are in or near the primary position.

(5) The apparent size diminishes continuously as the eyes move above or below the primary position.

(6) The full illusion is, therefore, realized when the eyes are maximally raised or lowered (about 40°) above or below the primary position.

(7) These generalizations are based upon the observations of mirrored moons compared with each other or matched to an artificial moon, and of mirrored suns compared with each other through a filter.

(8) The measurement of the apparent size of the moon is a function both of perceived size as dependent upon distance and of perceived size as dependent upon the elevation of the eyes (cf. Fig. 5).

(9) The convergence theory of the illusion (Zoth) is contradicted by the discovery that the apparent size of the moon is diminished in the lower quadrant, by the fact that monocular persons have the illusion, and by the fact that the small moon in elevation does seem to be further away than the large moon on the horizon.

(10) The torsion theory (Ames) accounts for the illusion in both upper and lower quadrants, but not for the monocular illusion, nor for the fact that the small moon in elevation appears to be relatively distant.

(11) The illumination theory (ours) does not show why the illusion should be as great as it is.

(12) The inhibition theory (Schur) remains uncontradicted, but needs the support of some specific neurological hypothesis as to how such inhibition might occur.

¹⁰ Schur, *loc. cit.*

REACTION-TIME DURING DISTRACTION AS AN INDICATOR OF ATTENTION-VALUE

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Studies of attention-value of stimuli, particularly in the field of advertising, have used as criteria principally: (1) Subjective estimates by rank order; (2) memory tests, including recognition- and recall-tests; and (3) the observation of eye movements and fixations. Many approaches to the measurement of attention have, on the other hand, depended upon reaction-time, and variations of the reaction-time method as criteria of the degree of attention at the time of the reaction. The present study represents an attempt to apply reaction-time in a new way as a criterion of the attention-value of color in advertisements.¹

An excellent review of work done on color in advertising, beginning with the work of Gale in 1900,² can be found in Poffenberger's *Psychology in Advertising*.³ The more recent work is that of Nixon.⁴ Using his "method of visual fixation," Nixon concluded that "it seems probable . . . that colored advertisements attract attention somewhat strongly for a very brief period, after which color as an attention and interest factor ceases to exert any marked influence." He reports a "very, very slight and unreliable tendency" for the colored advertisements to fail to hold attention after a few seconds exposure.⁵ In regard to memory tests, however, he states that "the chances are that the colored (advertisements) will excel in recall." The differences, it should be added, were not particularly reliable.⁶

Investigations into the relation of degree of attention to reaction-time considerably antedated the applied psychological investigations in advertising. Obersteiner in 1879 held that "retardation of the reaction stands in inverse proportion to the intensity of attention."⁷ Reaction-time was

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¹ From the Psychological Laboratory of the University of Nebraska. This study is based upon part of a master's thesis by the junior author, a copy of which is filed in the University of Nebraska Library.

² H. Gale, *Psychological Studies from the University of Minnesota*, 1900, 55 ff.

³ A. T. Poffenberger, *Psychology in Advertising*, 1925, 260-271.

⁴ H. K. Nixon, Attention and interest in advertising, *Arch. Psychol.*, 9, 1925, (no. 72), 54.

⁵ *Ibid.*, 53.

⁶ *Idem.*

⁷ H. Obersteiner, Experimental researches in attention, *Brain*, 1, 1879, 439-453.

used as a criterion with varying success, by Wundt,⁸ Swift,⁹ Cattell,¹⁰ Geissler,¹¹ and Dallenbach.¹² Woodrow, in a very thorough study of the relation of reaction-time to attention stated that the evidence that reaction-time does vary with degree of attention is "so overwhelming that it is unnecessary to argue the matter."¹³ Having expressed this view, however, he proceeded to develop and to establish his "detraction-time" technique, which he demonstrated to rest upon the principle that "the absolute detracting effect of a given detractor of attention varies inversely with the degree of attention upon which the detractor acts."¹⁴

PROBLEM

In some preliminary experiments, we found that with our stimulus material neither the absolute simple reaction-time nor Woodrow's detracting-time were adequate measures of attention-value. We consequently developed the new procedure which represents a reversal of the visual-attention reaction-time technique. Instead of measuring the attention-value of an advertisement by reaction-time to it in the presence of distractors, we proposed to let the advertisement itself be the distractor, and to measure the attention-value of the distractor by the amount of increase in reaction-time to a constant stimulus. The constant stimulus was a buzzer, sounding at an intensity that was not far above the stimulus-limen. The special problems to which we directed ourselves are: (1) Will a complex stimulus, such as an advertisement, be sufficiently distracting as to increase the reaction-time to a faint sound stimulus? (2) Will such an increase in reaction-time be related to the attention-value of the distractor? (3) Will the distractor effect different increases in reaction-time at different times during the observation of the advertisement and so indicate waxing and waning of attention to it? (4) Will the method show any consistent difference in the attention-values of colored versus uncolored advertisements? Our experiments answered some of these questions, but not all, and demonstrated the technique to be a sensitive indicator by virtue of

⁸ W. Wundt, *Grundzüge der Physiologische Psychologie*. 1st ed., 1874, 745-749.

⁹ E. J. Swift, Disturbance of the attention during simple mental processes, this JOURNAL, 5, 1892, 1-19.

¹⁰ J. McK. Cattell, The time taken up by cerebral operations, *Mind*, 11, 1896, 220-242.

¹¹ L. R. Geissler, The measurement of attention, this JOURNAL, 20, 1909, 473-529.

¹² K. M. Dallenbach, The measurement of attention, this JOURNAL, 24, 1913, 465-507; The measurement of attention in the field of cutaneous sensation, *ibid.*, 27, 1916, 443-460; E. E. Cassel and K. M. Dallenbach, An objective measure of attributive clearness, *ibid.*, 29, 1918, 204-207.

¹³ H. Woodrow, The measurement of attention, *Psychol. Monog.*, 17, 1914, (no. 76), 8.

¹⁴ *ibid.*, 99.

the systematic differences found among reaction-times under distraction. Some subsidiary questions arose during the course of the experiments, one or two of which will be treated in this report.

METHOD AND PROCEDURE

Materials. The stimuli were advertisements of the full-page type, selected from the 1936 and 1937 issues of the *Women's Home Companion*, *Delineator*, and *McCall's* magazines. All were in color, and since they were to be photographed both in Kodachrome and in black-and-white, care was taken to use only those with sufficient brightness contrast for good photographic results. The photographs were taken on 35 mm. film and mounted in 2 x 2 in. lantern slides.¹⁵ Thus, there were 100 slides in all, the same advertisement appearing in colored and uncolored form. Great pains were taken to equate colored and uncolored stimuli with regard to total brightness effect. Ordinary light meters were not found adequate for this purpose. We therefore resorted to subjective methods of equating them. Three judges observed the slides in successive pairs projected on the same screen. They first equated the pairs, colored and uncolored for total brightness effect, and then matched all pairs with a single standard. A rheostat in the circuit of the projection lamp controlled the illumination. The judges were very consistent with one another and with themselves at different times, so we felt that the stimuli were fairly well equated for total brightness effect. Brightness contrasts within an advertisement were approximately the same on colored and uncolored stimuli. The addition of color contrast to the colored stimuli had to be accepted as a natural part of the colored advertisement and if effective for attention its potency is merely added onto that of the mere presence of color.

Apparatus. *S* was seated in a small, unlighted room, and faced a 21 x 24-in. ground-glass window which served as a projection screen. The room was free from all disturbing distractions. The projection lantern was located in *E*'s room and mounted within a light-proof housing that fitted around the projection screen. The screen was covered with a cardboard frame to fit exactly the projected advertisements, leaving no diffusion or uneven margins. The slides themselves were also carefully framed with Scotch tape for the same purpose.

The timing device for measuring reaction-times was a Stoelting impulse counter in which the unit on the scale is 8.33 ms. The timer was connected in series (using an 8-v. transformer) with *S*'s key and with *E*'s key. The sound stimulus was the sound of the timer itself. The instrument was enclosed in a wallboard box to make its buzzing sound just audible to *S*, and this intensity was maintained throughout the experiments. *S* was instructed to press his key down when the advertisement came on the screen and to react to the buzzer by a release movement. Thus when *E* desired to give the sound stimulus he closed his switch, starting the timer. *S*'s release of the key broke the circuit and stopped the timer. The method had the advantages of simplicity and accuracy.

A recognition-test was developed to measure differences in retention of the colored and uncolored advertisements after 24 hr. This was intended to give us a criterion of attention-value that has already been employed, with which to compare our re-

¹⁵ We are indebted to N.Y.A. assistance for photographic preparation of the lantern slides.

action-times under distraction. For this purpose, short sentence descriptions of 34 of the advertisements were prepared. An additional 34 descriptions of advertisements not presented were prepared to mix with them for the recognition test. The recognition-test was administered 24 hr. after the presentation of the advertisements.

Procedure. Twenty S's, 8 men and 12 women, were employed in these experiments, all except 2 being selected from classes in beginning psychology. Each advertisement was seen by every S in both its colored and uncolored form. In one series (Series A) advertisements 1 to 25 were colored and advertisements 26 to 50 were uncolored. They were presented in the following counterbalanced order, 'C' denoting colored and 'U' uncolored: C U C U C U C U C U C U C. The order was repeated through the 50 advertisements. In the other series (Series B) advertisements 1 to 25 were uncolored, and advertisements 26 to 50 were colored. The order of presentation of colored and uncolored slides was reversed. Half the Ss (Group I) saw Series A at one sitting and Series B on the following day. The other half (Group II) saw Series B at one sitting and Series A on the following day. In the manner just described the effects of certain extraneous factors, such as size, contrast, human interest, and novelty were controlled.

Instructions. The instructions to the Ss were as follows:

You are going to be shown a series of advertisements. When an advertisement is flashed on the screen, press down the reaction-key. While you are looking at the advertisement a buzzer will sound. As soon as you hear the buzzer, release the key, then press it down again so you will be ready for the next reaction. Pay close attention to the advertisements. You will be given a test over what you have seen.

It was felt that the last part of the instructions was important in order to insure a less ambiguous attitude toward the advertisements.

Every S came for three experimental periods, on three consecutive days. In the first period S was given 200 practice reactions with the buzzer alone, without any advertisements being presented. After a 5-min. rest period 100 more reactions were taken to determine S's average reaction-time without distraction. In this series the reaction-stimulus was given in groups of 25 trials with an irregular interval of 11 to 15 sec. between stimuli. No separate warning signal was given before each stimulus.

In the second and third experimental periods the advertisements were presented. A practice series of 25 reactions to the buzzer alone was given, following which the advertisements were shown. During the exposure of each advertisement the buzzer was sounded three times, once each during the intervals of 3-6, 12-15, and 21-24 sec. after the first moment of exposure. The position of the reaction-stimulus within these three intervals was varied at random. Each advertisement was shown for a total of 25 sec.

RESULTS

Reaction-times. A summary of the average reaction-times obtained under the various conditions of our experiments will be seen in Table I.

We are especially to note whether the reaction-times under distraction of the advertisements are longer than normal; whether there is any difference in reaction-times under the influence of colored versus uncolored advertisements; whether the latter difference, if any, is correlated with a

corresponding difference in recognition scores; and whether there is any systematic change in reaction-time as *S* continues to observe the advertisements during the 25-sec. period.

It is quite clear that the average reaction-times taken during observation of the advertisements are longer than normal. This result held true for every *S* as well as for the averages for all *Ss*. Even the shortest reactions, obtained during the third interval (21-24 sec.), are about 10 units greater than normal. The longest reactions, obtained during the first interval (3-6 sec.), are about 20 units greater than normal. In terms of milliseconds

TABLE I

GENERAL SUMMARY OF NORMAL REACTION-TIMES, REACTION-TIMES WITH DISTRACTION, AND RECOGNITION SCORES

(Reaction-time unit=8.33 ms.; C=colored; U=uncolored.)

Interval	Group I (Av. normal RT=34.4 units)				Group II (Av. normal RT=33.1 units)			
	2nd sitting		3rd sitting		2nd sitting		3rd sitting	
	C	U	C	U	C	U	C	U
1	56.8	60.8	51.7	50.4	54.7	54.9	52.2	53.4
2	46.8	46.4	43.6	43.7	43.9	45.1	45.4	45.1
3	46.6	46.5	44.1	44.4	42.3	43.1	45.5	44.8
D ₁ -D ₂	10.0	14.4	8.1	6.7	10.8	9.8	6.8	8.3
CR σ	7.3	8.5	5.8	5.0	5.3	14.6	4.9	4.9
Recog.-scores			8.4	2.4			7.2	6.0
Diff.				6.0				1.2
CR σ				2.1				0.4

the difference is about 83 ms. in the first case and 167 ms., in the second. To what may the differences be attributed? Since the only difference in the conditions is the presence of the advertisements and the instructions to observe them for later recall in the one case, and their absence in the other, the inference is hardly escapable that the competing influence of the advertisement detracts from attention to the reaction to the buzzer. We do not know what the real functional relationship is between the degree of attention demanded by the advertisement and the change in reaction-time. All we can say is that the longer the reaction-time the greater the distracting effect of the advertisement.

As to the second question, any difference between the colored versus uncolored stimuli, we find no marked or consistent differences in Table I. This holds true at all intervals and sittings. The means for the data of presentations 1 and 2 combined, first interval, 1, are 53.8 units for colored

and 54.9 units for uncolored advertisements (see Table II). The difference of 1.1 units is in favor of the *uncolored* advertisements, but it cannot be regarded as statistically significant. From this we should have to conclude either that there is no difference in attention-value (or distracting power) as between colored and uncolored advertisements or that our new method was incapable of discovering it. In view of the strong general detracting effect shown for all advertisements, we are disposed to favor the first of these two inferences. The fact that there is a marked change in reaction-time during the time of observation of the advertisement, as we shall see later, shows the possibility of the method's differential power and leads us to the same conclusion.

TABLE II
COMPARISONS OF AVERAGE REACTION-TIMES DURING DISTRACTION WITH COLORED VERSUS UNCOLORED ADVERTISEMENTS DURING THE DIFFERENT PRESENTATIONS AND INTERVALS
(Reaction-times in units of 8.33 ms.)

Presentation	First interval			Second interval		
	C	U	Diff.	C	U	Diff.
First	55.7	57.8	2.1	45.4	45.8	0.4
Second	51.9	51.9	0.0	44.5	44.4	-0.1
Both combined	53.8	54.9	1.1	44.9	45.1	0.2

Recognition-tests. The data from the recognition-tests are somewhat different. The recognition scores were based upon the number of correct recognitions minus the number of advertisements not recognized. The average scores were 7.8 and 4.2 for the colored and uncolored stimuli, respectively, when data from Groups I and II are combined. Twelve Ss had a higher recognition-score for colored stimuli, 5 had a higher score for the uncolored, and 2 had equal scores for the two kinds of stimuli, one S not having taken the recognition test. The critical ratio of the difference for all Ss combined is 1.79, which is below the level of significance.¹⁶ The strong suggestion of a general difference in favor of the colored advertisements here, as contrasted with a slight reversal of the difference, if anything, in the case of the results of the distraction-test, is in line with the earlier findings.¹⁷ It is our view that recognition-scores, as compared with other criteria of attention to stimuli, are heavily contaminated with factors other than differences in attention and hence cannot be used as unadulterated indicators of attention-value of stimuli.

There is a possibility that the distraction-test failed to discriminate be-

¹⁶ In all tests of significance we have observed the small-sample principles.

¹⁷ Particularly those of Nixon previously cited.

tween the two kinds of stimuli in a manner consistent with the recognition-test due to the fact that the attention of the *Ss* was too nearly maximal in both cases. The instruction for *S* to give close attention in order to be able to recall them may have brought this about. The factor of hue would then be relatively impotent to produce an increment of attention. It might even be that the *Ss* were set to give close attention to the uncolored advertisements as a compensatory measure because of a vague realization that uncolored material was harder to grasp and remembered than colored. In opposition to the last hypothesis is the fact that, if anything, they did retain more from the colored stimuli. In opposition to the idea that when attention to the advertisements is maximal there can be no increment due to color is the fact that during the later intervals of reaction, when the reaction-times regressed back toward the normal we cannot say that attention was maximal by any means, and yet the reaction-times tell the same story as regards any difference between colored and uncolored stimuli. We doubt whether attention was actually maximal during the first interval, for had it been so there would have been many failures to react. This was not the case. We are therefore brought back to a faith in the method as a fair criterion of attention and to the conclusion that colored stimuli have no clear advantage over uncolored ones as claimants for attention. It is true that Nixon's result showed a slight advantage for colored advertisements during the first few seconds. Our first reactions, coming 3-6 sec. after the beginning of the exposure may have come too late to catch this early difference. A more searching exploration of these early seconds is desirable, not only to determine any possible early difference between the two kinds of stimuli, but also to study the general early shortening of reaction-time as compared with the later decrement.

Decrement during distraction. The fact that the reaction-times were longer on the average during the first interval than during the second and third intervals has already been mentioned. The averages during the last two intervals are approximately equal, the grand averages being 45.0 and 44.7, respectively, as compared with an average of 54.3 for the first interval. A summary of the differences between intervals one and two is given at the bottom of Table I, along with the critical ratios for those differences. Under all conditions the differences are decidedly significant.

Such a decrement in the reaction-time between first and subsequent intervals could arise from two sources: (1) A decrease in distracting power of the advertisement as time goes on; and (2) a change in the set of the individual because of his reaction during the first interval. If the decrement is due to the second factor alone, then the reactions of *S* to a stimulus

during the second interval, none having been given during the first, should be as long as the usual first-interval reactions. In order to test this deduction a control experiment was introduced. Five new *Ss*, all but one of whom were graduate students, were given the reaction-time test during observation of one series of the advertisements. All procedures were the same as for the earlier *Ss* except that during half of the observations reactions were called for during the first interval as usual but during the other half no reaction was called for until the time of the second interval, that is, 12-15 sec. after the beginning of the observation. The two kinds of observations were given in counterbalanced order, 25 reactions being obtained under each kind for every *S*.

The results of the control experiment are given in Table III. The average

TABLE III
AVERAGE REACTION-TIMES DURING DISTRACTION WITH AND WITHOUT A
REACTION DURING THE FIRST INTERVAL
(Reaction-times in units of 8.33 ms.)

<i>S</i>	First interval	Second interval	Second interval (Control)
<i>L</i>	47.9	34.9	32.4
<i>C</i>	54.6	39.2	46.5
<i>ED</i>	50.1	38.1	41.8
<i>W</i>	44.1	33.2	32.7
<i>FD</i>	39.9	34.1	36.0
Mean	47.3	35.9	37.9
Diff.		11.4	9.4

reaction-time during the second interval when a reaction was called for during the first interval was 35.9 units. When no reaction was called for during the first interval, the average was 37.9. In general, there is only a slight tendency toward a lengthening of the reaction-time during the second interval when response during the first interval is omitted. The mean difference is only 2.0 units, to be compared with a difference of 11.4 units between average reaction-times for first and second intervals for these same *Ss*. Two of the 5 *Ss* actually have a slightly larger average reaction-time during the normal second interval than during the control second interval. A preponderance in favor of the second control interval such as exists in the total averages might be ascribed to the longer total preparatory interval (about 12 sec. as compared with about 3 sec.).

The results of the control experiment definitely point to our first hypothesis rather than to our second. More parsimoniously, they show that our second hypothesis is incorrect. Having no better theory than our first, we are inclined to take the decrease in reaction-time during observation of the advertisements to mean a decrement in attention. If this be true, the

degree of attention drops most rapidly during the first 12 sec. of observation and then more slowly, if at all, during the next 10 sec. At the end of the 25 sec. there is some attention to the advertisements remaining, since the reaction-time for the third interval is then still some distance from the normal reaction-time, or the reaction-time found without the presence of the advertisements. There should be a systematic study of the general picture of decreasing reaction-time, extending the time of observation beyond 25 sec. Such a study would probably yield some quantitative laws of attention under such circumstances. In view of the fact that *S*'s are really under a double instruction so far as attention is concerned, one can view his decrement in reaction-time as a continuous readjustment as regards these competing demands upon his reactive mechanisms.

Corollary results. (a) Effect of color. When comparing the decrements in reaction-time that occur with colored versus uncolored advertisements, some slight differences appear. These differences are noticeable in Table II. It appears that the decrements from interval one to interval two are greater for uncolored than for colored advertisements. The average decreases are 10.3 and 12.0 units for colored and uncolored stimuli, respectively, or an average difference of 1.7 units. This difference amounts to 14 or 15 ms., and though not statistically significant, it occurs quite consistently for different *S*s. If such a difference were to hold up in further tests, it would probably mean something with regard to the relative power of colored versus uncolored advertisements to hold attention. The same difference failed to show up in the second presentation of the advertisements, so we are unjustified in urging any interpretation in this particular use of the attention-test. In other situations, however, we might expect that the relative holding powers of different kinds of stimuli could be determined by our method.

(b) Factor of familiarity. As in most experiments on attention, adaptation effects almost inevitably set in. *S*s form habits of attending which may detract from differences found early in the experiments. By comparing the results during first and second presentations of our material we can test the effect of adaptation or familiarity in our test.¹⁸ In this we will confine our attention to the first interval, where the greatest evidence of attention-value were found. The mean reaction-time for the first presentation was 56.8 units, and for the second 51.9 units. The difference is 4.9 units, or about 40 ms., with a critical ratio of 4.6, which is very significant.

¹⁸ It should be remembered that the *S*s saw the same advertisements during the third sitting as in the second, except that the colored advertisement seen in sitting 2 were uncolored in sitting 3, and conversely.

This decrease could possibly be attributed to a practice effect, but against this hypothesis is the fact that for the second and third intervals no such differences appear. Since there was still much room for improvement, as compared with the normal, undistracted, reactions during the second and third intervals, and no apparent learning appeared in them, we are inclined to rule out learning as the explanation of improvement in reaction-time during the first interval. We are more inclined to attribute the change during the first interval to a lowered attention during the second presentation, due to the factor of familiarity. This decrement, though smaller, is of the same kind as occurs during the same period of observation, even though the advertisement as seen the one time was colored and the other time uncolored. Had the repetition been identical, the decrement might have been greater than it was. We are inclined to take all such indications as support for the validity of the method and of its rather sensitive qualities for detecting differences in attention to stimuli.

SUMMARY AND CONCLUSIONS

A new method for measuring the attention value of stimuli was developed and tested with colored versus uncolored advertisements as stimuli. The principle of the method is that reaction-time to a faint sound stimulus is lengthened in proportion as the individual attends to another stimulus which he is instructed to observe in order to recall or recognize it later. The results showed that:

(1) Reaction-time is considerably lengthened while observing an advertisement during a 25-sec. period.

(2) There is a decrease in reaction-time during the course of the observation of the advertisement, rapid at first then slow or stationary, but during a 25-sec. interval the reaction-time does not reach the normal level which prevails without the competing effect of the advertisement. This decrement appears not to be due to the fact that *S* makes reactions repeatedly through the interval, but rather seems related to the waning of attention to the advertisement.

(3) The reaction-times were significantly shorter during a 3 to 6-sec. interval upon a second showing of the same advertisements, probably due to a lowered attention and not to practice.

(4) Reaction-times were about the same during observations of both colored and uncolored advertisements, if not slightly longer during the uncolored. There was slightly, but not significantly, more decrease in reaction-time during uncolored than colored stimuli.

(5) Recognition-tests gave colored advertisements a higher average score than uncolored ones, though the difference was not statistically significant. In line with earlier findings, this suggests again that recognition-tests are not pure measures of attention-value.

(6) All indications point to the fact that the new method of reaction-time during distraction by observation is really a measuring instrument, and perhaps a delicate one, of attention-value of stimuli. A checking against other established methods is needed, however, before its validity can be thoroughly established.

OCULAR PATTERNS AS AN INDEX OF THE ATTENTIONAL VALUE OF SIZE

By HERMAN F. BRANDT, Drake University

Numerous techniques have been employed to evaluate the attentional values of size. Among those most commonly employed are the recall, recognition, and coupon return methods. The recall-method consists simply in having a *S* look through a magazine and later attempt to recall respective advertisements. The amount of recall and the variable in question are computed to arrive at the ratio between the two. The recognition, another method, is similar to the recall technique except that the *S* is required to identify advertisements in the second examination seen during his first observation. A third method is based on the relation between certain variables and the number of coupon returns of a certain advertisement in a publication.

Other methods have been designed to arrive at conclusions dealing with the problem as stated above. These may be identified by such terms as tachistoscopic, visual-fixation, and techniques in which illumination is varied. Only brief reviews of representative studies are made to illustrate different experimental approaches to evaluate the variable of size as a determinant of attentional advantage.

Recall. Scott employing the recall-method concludes that the half page including twice the area of the quarter-page secures three times the attention, and the full page which is four times the size of the quarter-page receives almost seven times the attention.¹

Recognition. Strong, resorting to the recognition method, finds that the half page is only forty per cent better than the quarter, and the full page is a little more than twice as good as the quarter-page. His findings suggest that attention value is proportional to the square root of the area.²

Coupon return. Rudolph concludes on the basis of 880,620 replies from 441 magazine advertisements that the size of space and reply ratios is 69.32, 73.01, 100.00, and 150.13, for one-half page, two-thirds page, one page, and double page respectively.³

Here as in similar studies the reply ratio lagged behind the increases of size. Rudolph found that in some products the effectiveness varies in direct proportion

* Accepted for publication May 21, 1940.

¹ Quoted from H. E. Burt, *Psychology of Advertising*, 1938, 159.

² E. K. Strong, Jr., The effect of size of advertisements and frequency of their presentation, *Psychol. Rev.*, 21, 1914, 136-152.

³ F. H. Rudolph, *Four Million Inquiries from Magazine Advertising*, 1936, 25.

to size, while in other cases the smaller advertisements were almost as effective as the larger ones.

Basing his conclusions on 1,395,800 returns from 907 advertisements, Starch concluded that the space, size, and returns yielded ratios of one page:one-half:one-fourth:one-sixth page, to 100:50::25:17.⁴ According to his findings the ratios would seem to indicate that the returns increase as space increases making the relation linear.

Falk's study of the returns from advertising conducted by Gordon Van Tine Company in which over one million returns from keyed advertisements were analyzed seems to show that neither the linear rule nor the square-root rule held.⁵ He evaluated the relative efficiency of different size advertisements by computing the cost in terms of mail-order returns. His cost ratio of one-half page: one page: two-page is 6:10:16, and would seem to indicate that the relative effectiveness of an advertisement becomes smaller as the size increases. Three different mediums of selected advertisements yielded ratios of 100, 83, 65; 100, 65, 48; and 100, 55, 43; for one, one-half, and one-quarter pages, respectively. In case of increased volume the large space was more effective than the smaller one.

Tachistoscopic. Adams experimented with colored-paper squares (colors used for identification only), 1, 1½, 2, and 3 in. sq.⁶ This gave areas in the ratio of 1, 2.25, 4, and 9. A tachistoscope was employed to expose the materials. Four squares of varying size were mounted on the exposure-card in the form of quadrants, and the color seen first represented the size of the paper seen first. (All colors used appeared equally often in every position and size in order to average out all variables except size.) A record was made of the size seen first and the comparative frequency with which the various areas were seen first was established as an index of attentional value. Such figures were obtained for all Ss and then reduced to ratios.

Adams concludes that an area four times larger than another gets only about twice the attention, while the 3 in. sq. (nine times the area) has an attention ratio of 2.75. To get double the attention it was necessary to increase the area four times, and to get three times the attention the area had to be increased nine times.

Visual fixation. Hackman and Guilford employed a certain type of eye-movement technique to determine the degree of attentivity (attention).⁷ Their Ss looked at a rack on which were displayed two advertisements while the experimenters looked through a one-way screen and recorded the eye-movements. They noted the proportionate number of times the attention of the eyes were directed to one or the other advertisements. When, for example, a full page and a half page advertisement were presented simultaneously, they discovered by means of their technique the relative amount of time devoted to each layout.

The results indicated that the first fixation went to the full page 16% more frequently than to the half page. During the first 10 sec., the S spent 23% more time looking at the larger advertisement, and for a 30-sec. period he spent 32% more time. They conclude that the larger advertisement received considerably more

⁴ D. Starch, *Analysis of 3,000,000 Inquiries*, 1927, 1-30.

⁵ A. T. Falk, *Analyzing advertising returns*, *Harvard Bus. Rev.*, 1929, 312-317.

⁶ H. F. Adams, *Advertising and Its Mental Laws*, 1921, 107.

⁷ R. B. Hackman and J. P. Guilford, A study of the visual fixation-method of measuring attention-value, *J. Appl. Psychol.*, 20, 1936, 44-49.

attention than the smaller one, but it did not by any means command the attention given to the smaller stimulus-card.

Illumination varied. Curtis and Foster attempted to compare the attention-value of size by varying the intensity of illumination of three stimulus-areas—Greek crosses, 28, 56, and 112 sq. cm., respectively, in size.⁸ Using the middle size of these areas as a standard, with a constant intensity of illumination, the other two areas were each in turn compared with it, by means of a projection lantern with a tachistoscopic attachment, at varying intensities of illumination. The areas were exposed horizontally with a fixation-point midway between them. The time of exposure was 110 ms., and the method of constant stimuli was used in the experiment.

Three *O*s (*F*, *B*, and *D*), all trained in observation, served in study. Every one gave judgments in 200 series: 50 with each of the two comparison crosses in each of the two spatial positions to the right and left of the standard. The results were as follows. For *D*, size seemed to have an effect. To be equal to the standard in attention-value, the smaller cross had to be greater than the standard in illumination and the larger cross had to be less. For *B* and *F*, however, this relation did not obtain. One showed a negative effect of size while to the other size was apparently indifferent. Curtis and Foster consequently concluded that size was not a definite determinant of attention.

Bowman repeated the experiment of Curtis and Foster.⁹ She used the stimulus-areas employed by them but a different apparatus and a different method of procedure. Her apparatus was constructed to avoid the technical difficulties met by Curtis and Foster, and she determined the point of subjective equality of the attentional value of the stimulus-areas by the method of limits.

Four *O*s, trained in the observation of clearness (attention), served in four series of experiments: two in each space order with each of the variable stimulus-areas. The results for all the *O*s were constant in direction. The small cross to be as clear as the standard (which was twice its area) had to be more intensely illuminated, and the large cross (which was twice the area of the standard) had to be less intensely illuminated. Bowman found that there was a large space error. When this was canceled out by averaging the results of the two space orders, the conclusion, that size was a determinant of attention, was unescapable.

Because Bowman had varied the method of procedure—had used the method of limits in place of the constant method—it was necessary, before reaching a decision regarding her conclusions, that the experiment be repeated with Curtis and Foster's method of procedure. This was done, with an improved apparatus,¹⁰ by Dewey and Dallenbach.¹¹ These experimenters used circular openings, disks of light, instead of Greek crosses as the comparison-areas. These areas, two instead of three in number, were respectively 4 and 8 cm. in diam. They were exposed in a hori-

⁸ J. M. Curtis and W. S. Foster, Size versus intensity as a determinant of attention, this JOURNAL, 28, 1917, 293 ff.

⁹ A. M. Bowman, Size versus intensity as a determinant of attention, this JOURNAL, 31, 1920, 87 ff.

¹⁰ K. M. Dallenbach, An apparatus for the study of the conditions of clearness, this JOURNAL, 34, 1923, 94 f.

¹¹ Doris Dewey and K. M. Dallenbach, Size versus intensity as a determinant of attention, this JOURNAL, 35, 1924, 121-125.

zontal position, to the right and left of a fixation-point which was centered equidistant (10 cm.) between them. The large and the small areas were taken in turn as standard and variable in each of the two spatial positions.

These results corroborated those obtained by Bowman. When the effect of position, which they too discovered to be large, was canceled out by averaging, they found that size was a determinant of attention. With the small area at 16 intensity-units of illumination, the large area needed only 5.7 units for *D*, 6.7 for *G*, and 7 for *W* to equal it in clearness. Similarly, when the large area was illuminated at an intensity of 16 units, the small one needed 40.8 units for *D*, 47.8 for *G*, and 48 for *W*.

Purpose.—The purpose of this study is to evaluate by means of ocular photography the relative attention-value of size. The location of fixations

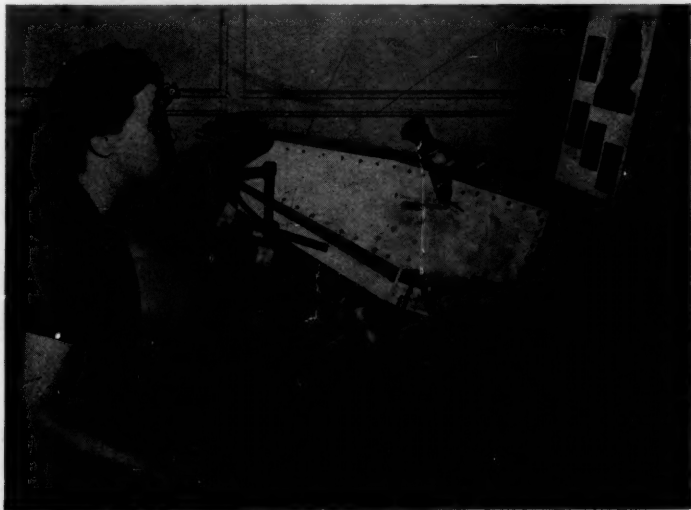


FIG. 1. APPARATUS IN USE.

and time spent in respective areas will be considered the unit of measurement and is expected to provide an objective criterion for the evaluation of the attentivity of the variable under consideration.

Procedure. (a) *Exposure cards.* Two sets of exposure cards were used in this experiment. The first set of four cards (one of which is shown in Fig. 2) consisted of five designs mounted on a card 10.5 x 13.5 in., i.e. the size of the page of a standard magazine. One of the designs has four times the area of the other four.¹³

¹³ The size of the large area, designated as Area 5, was 6.6 x 6 in. The smaller areas designated as Areas 1, 2, 3, and 4 were 3.3 x 3 in.

The second set of four cards (one of which is shown in Fig. 3) was identical with the first set with the exception that the individual designs were replaced by pictures. The large design and picture were rotated (as shown in Figs. 4 and 5) so that the Ss viewed it in all four positions on the card and the smaller ones were

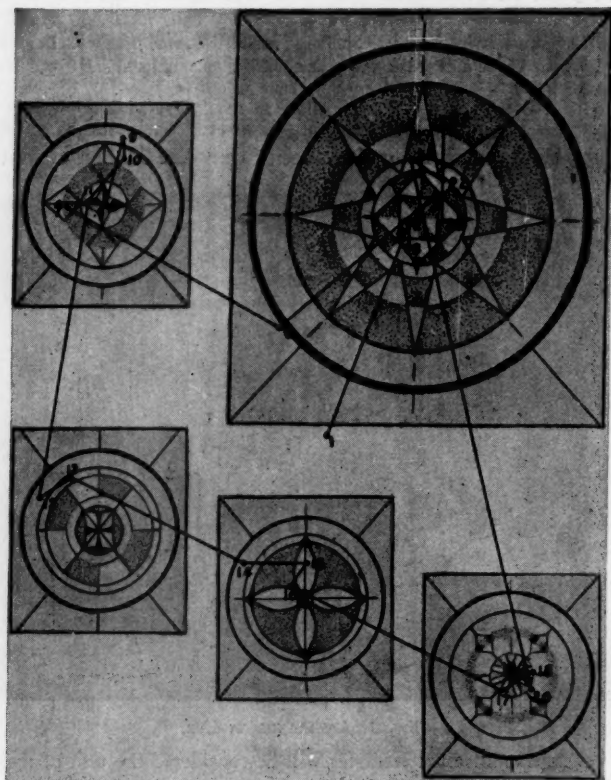


FIG. 2. OCULAR PATTERN OF S 19

reversed in their order for each viewing. This procedure eliminated the variable of position.

(b) Subjects. A total of 200 college students (119 men and 81 women), chosen at random, served as Ss. Of these, 61 men and 39 women observed the designs, while 58 men and 42 women viewed the pictures. Every S was given once the following instructions: "You will see a number of pictures or designs, look at

them as naturally as you would when reading a magazine at home." Although the reading time for this card was limited to 10 sec., the Ss were not informed in advance of the time-limit. Each card was inspected by 25 Ss. No S was shown more than one card.



FIG. 3. OCULAR PATTERN OF S 24

(c) Apparatus. A bidimensional eye camera was employed in this experiment.¹⁹ After the eye movements of the Ss were photographed, every record was projected and the ocular pattern was superimposed on the original content revealing the location and sequence of the fixations and relative amount of time spent on each area.

¹⁹ H. F. Brandt, A bidimensional eye movement camera, this JOURNAL, 49, 1937, 666-670.

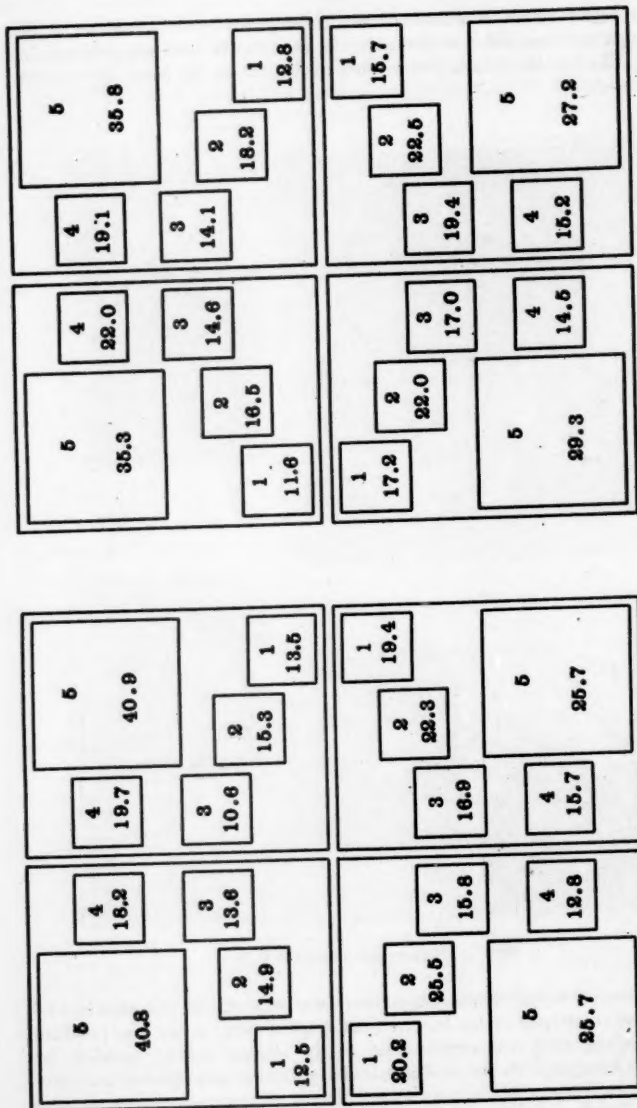


FIG. 5. PERCENTAGE OF TOTAL TIME SPENT ON RESPECTIVE PICTURES

FIG. 4. PERCENTAGE OF TOTAL TIME SPENT ON RESPECTIVE DESIGNS

Results. (a) Ocular patterns. While a total of 200 Ss were used in this investigation, the ocular patterns of only 2 Ss are presented here to illustrate the major characteristics. (Fig. 2 represents an ocular pattern of one of the four exposure cards of the designs, and Fig. 3 represents one of the pictures.)

The relative time spent in respective areas of each exposure-card is indicated in Figs. 4 and 5. Here as in previous studies position is a potent

TABLE I
TOTAL TIME SPENT IN AREAS 1, 2, 3, 4, AND 5 OF THE DESIGNS
(Time in sec.)

Position of large area	Area				
	1	2	3	4	5
Upper left	113.8	135.4	123.4	165.6	371.6
Upper right	123.1	139.4	96.7	179.8	373.6
Lower left	190.9	240.5	148.8	120.8	243.1
Lower right	176.9	203.2	154.5	143.0	234.4
Mean	151.2	179.6	130.8	152.3	305.7

factor of attentivity.¹⁴ Considerably more time is spent in the large areas when placed in the upper half of the field than when located in the lower part of the exposure-card. Position as indicated in Figs. 4 and 5 affects

TABLE II
TOTAL TIME SPENT IN AREAS 1, 2, 3, 4, AND 5 OF THE PICTURES

Position of large area	Area				
	1	2	3	4	5
Upper left	106.3	150.1	132.9	200.2	321.6
Upper right	144.9	163.6	127.3	172.4	322.9
Lower left	154.6	197.9	153.3	130.4	264.4
Lower right	148.4	212.8	183.0	143.8	256.8
Mean	131.1	181.1	149.1	161.7	291.4

the attentivity of the smaller areas also but in no case does the time spent on the smaller areas equal that of the time devoted to the larger ones.¹⁵

Table I and II represent the total time spent on respective areas for both the design and pictured areas. From the analysis of the data in the tables a significant difference in time spent exists between the attentional value of the smaller and larger areas of the exposure cards.

¹⁴ Brandt, Ocular patterns and their psychological implications, this JOURNAL, 53, 1940, 260-268.

¹⁵ The number in the upper half of each area in Figs. 4 and 5 represents the respective design or picture area, while the numbers below represent the relative time in percentage.

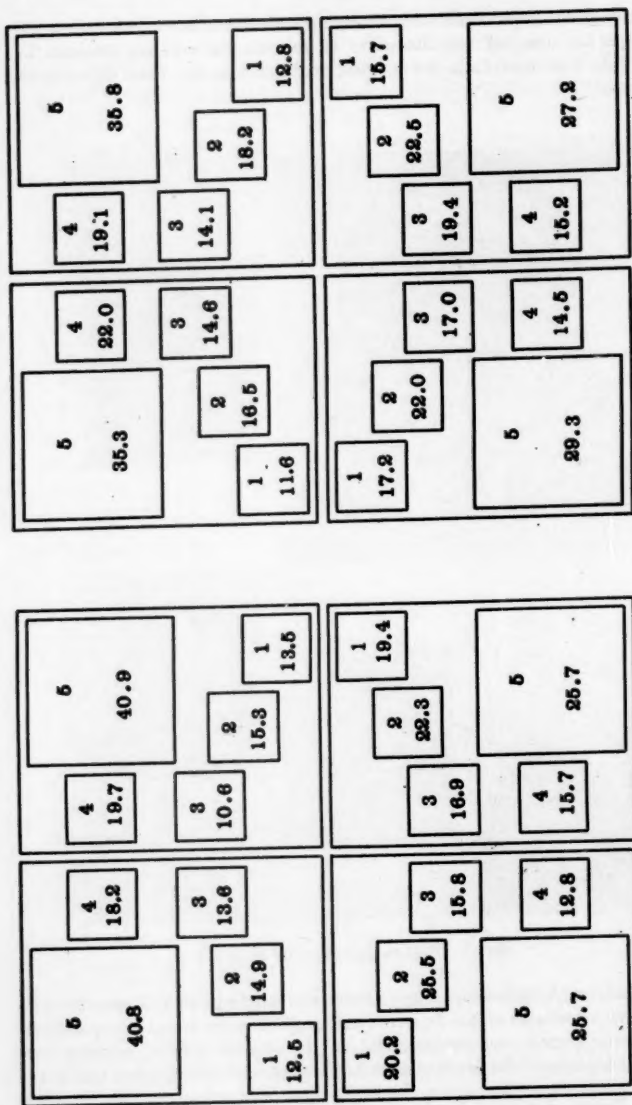


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It is apparent from Table III that approximately 17% of the total time was devoted to each of the small areas while 32% was devoted to the larger one.

This difference represents an attentional ratio as based upon time spent on small and large areas of 1:2 while the size is in the ratio of 1:4.

The results of this study show that regardless of the position or the

TABLE III
PERCENTAGE OF TOTAL TIME SPENT IN AREAS 1, 2, 3, 4, AND 5 REGARDLESS OF POSITION
AREA

Exposure cards	1	2	3	4	5
Designs	16.2	20.4	14.1	16.4	32.9
Pictures	14.3	19.8	16.3	17.7	31.9

character of the mass, significantly more time is spent on the larger areas than on the smaller ones.

Summary and conclusions. The present study is an attempt to evaluate by means of ocular photography the attentional value of size. The appraisal of the attentivity of size by means of ocular photography is more objective than many of the techniques of analysis employed earlier. Psy-

TABLE IV
MEAN DIFFERENCE OF AREAS 1-4 AND 5 AND THE CRITICAL RATIOS OF DESIGNS AND PICTURES

Exposure cards	Areas	M	S.E.	M _{Dif.}	S.E. _{Dif.}	C.R.
Designs	1-4	153.49	3.57	152.17	13.79	11.03
	5	305.66	13.22			
Pictures	1-4	155.74	2.94	135.69	12.18	11.01
	5	291.43	11.95			

chology employing this new device (as illustrated by this study) aims to minimize the reliance on introspective measures and establishes an objective criterion of evaluation.

Since some of the investigations on size vs. attentional value, reviewed in this study, differ in their conclusions we may surmise that the discrepancy is due to one or more of the following reasons: (1) that different experimental procedures were employed; (2) that variables other than size were not adequately eliminated; and (3) Or that both procedure and variable control were responsible. From all indications it seems to follow that the more adequately the size variable was isolated the greater was the lag of attention relative to increased size areas.

The results of this study are corroborated by the studies of Strong, Dewey and Dallenbach and supports the conclusion that large areas have attentional advantages over smaller ones, but that such advantage lags as the size of the area is increased. The findings also indicate that position and the character of the area are independent variables and potent determinants of attracting and sustaining attention.

Each of the 200 ocular patterns obtained in this study tell a story in terms of fixations and excursions, thus indicating location, duration, and sequence of fixations as well as direction, distance, and frequency of excursions. Each implies a type of attention or interest with its accompanying psychological processes of selection, elimination, comparison, and interpretation.

The results coincide rather closely with the square root effect advocated by Strong, who on the basis of his findings theorized that an area four times the size of another would have only about twice the attentional advantage. What the difference of attentivity and size would be for size ratios of 1:2, 1:3, 1:9, 1:12 or some other combination cannot be determined from the results of this investigation. Subsequent studies will provide information which will have a direct bearing upon this problem.

Implications. The writer assumes that the time spent in respective areas of a given field is indicative of the relative attention of an individual's observation. He accepts the term 'attention' simply as that which for the moment seems to be the most vivid portion of the conscious state. Due to the relevance of the source of attentional experience, the writer contends that in any form of sensory appeal two types of incentives operate: one, the mechanical consisting of sensory data such factors as position, size, contrast, isolation, color, or the novelty of the stimulus; and the other, the psychological such as curiosity, interest, or a purpose resulting from the memory residues and present interpretation of the physical stimulus.

On the basis of such an explanation the character of visual experience known as visual consciousness would evidently consist of a fusion of data coming from the eye on the one hand and from the mind on the other. Attentivity and cognition operate simultaneously for the most part, but it is likely that the former appeal takes precedence over the latter in the initial period of observation as is the case in this study. In classified advertising, no doubt, the purposed incentive has greater potency and therefore yields returns to the observer in terms of ends sought.

This study has made no attempt to separate the two types of incentives, but assumes that in the initial period of observation, attention is more the result of sensory appeals than of central processes.

Intense stimuli due to the stimulation of a larger number of nerve fibers evidently release greater nerve energy causing as a result an intensified and prolonged activity. The assumption is that in the presence of situations arousing attention the area fixated visually may be considered the object of attention. Of two or more attention provoking situations presented simultaneously, the one eliciting the longer periods of visual fixation over a period of time may be taken to possess the greater attention value. Evidently the larger area has the advantage of greater magnitude, less distraction, and added prestige over that of the smaller ones.

On the other hand, the reason the larger area does not increase the attentivity in proportion to the increase in size may be due to the division of the S's time between areas, not in proportion to size but in proportion to respective items in the area.

Even if the ratio between attention and size is only a symbol of relations, it nevertheless may serve as a basis for the evaluation of space. Size and dollars and cents have much in common in advertising, and it follows that good accounting would be based on an accurate knowledge of the relative effectiveness of physical variables. Certain factors essential in an art production are physical in nature and may be controlled by the designer if their relative attentivity is known. A relative attention value may thus be attached to various sizes which may determine an esthetic as well as an economical aspect of the problem.

CHILDREN'S REACTIONS TO ELEMENTS OF SIMPLE GEOMETRIC PATTERNS

By CLARENCE LEUBA, Antioch College

In 1929 Köhler reported some experiments by Hertz with two jay birds indicating that these birds reacted to simple geometric configurations (circles, lines, etc.) as unanalyzed wholes.¹ The birds failed to react selectively to the elements of which these configurations were composed. Though the jays would go directly to a single inverted flower-pot, under which they had seen Miss Hertz conceal some food, tip the pot over, and secure the food, they were unable to go directly to a flower-pot concealing food when that pot was incorporated in some configuration of flower-pots, as in a line or circle of them. The jays seemed to react to these configurations as unanalyzed wholes; when confronted with one of these flower-pot patterns, they tipped over whatever pot they chanced upon first. If, however, the food-concealing pot was on the outside of a line or circle of pots, or if there was a sufficiently wide space between it and the other pots in the configuration, the birds usually picked it out correctly at once.²

The experiments to be described in this article were designed to discover whether young children perceived common geometric configurations like lines, circles, squares, and triangles, even when these were composed of discrete units, as unanalyzed wholes in the same fashion as the jays.

PRELIMINARY EXPERIMENTS

Hertz's experiment with the jays was adapted for use with a young child (the author's son) as follows: while *D* (the child) sat in a chair about 6 ft. away, *E* (the father) placed a bit of chocolate inside one of a series of small safety match boxes ($\frac{3}{4} \times 1\frac{1}{2} \times 2\frac{1}{4}$ in.) from which the box part containing the matches had been removed. The match boxes were upside down, plain under side showing, and were arranged in one of three configurations: a straight line, usually parallel to the wall in front of the child, a circle, or a square. The number of boxes used in any one configuration varied from 6 to 14; the distance between two adjoining boxes was never more than $\frac{1}{2}$ in. *D* was fond of chocolate and always spontaneously followed the piece of chocolate as it was placed in one of the boxes. To make doubly certain of attention to this process, however, *E* always described it verbally and looked at *D* while the box containing the chocolate was being replaced in the pattern; *E* made sure that *D*'s eyes were on that box. To prevent *D* from maintaining a bodily orientation toward the box containing the bit of chocolate, he was then asked to turn away from the match boxes and with his back to them to wait 1 min. before seeking the chocolate.³ *D* was given several trials during each of three sessions when

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¹ W. Köhler, *Gestalt Psychology*, 1929, 158-162.

² A more recent and complete description of these experiments is given by Hertz in W. D. Ellis *A Source Book of Gestalt Psychology*, 1938, 238-252.

³ In children as well as in animals, bodily orientation is an important factor in reacting correctly to one of a number of objects. Cf. L. L. Emerson, The effect of

he was 29 mo. old, and during each of two sessions at 31 mo.

In nine trials with the boxes arranged in a line the number of boxes used varied from 6 to 14. Numbering the boxes from left to right, the chocolate was placed twice in box 3 and once in each of the following positions: 4, 5, 6, 7, 8, 10, and 14. Seven times *D* went to the wrong box and twice to the correct one. With the circle (the number of boxes again varied from 6 to 14) he made five wrong first choices and one correct. A square with three boxes on a side was, however, easy for him; he made three correct first choices and no wrong ones.

From *D*'s behavior it could reasonably be inferred that he perceived both the circle and the line as unanalyzed wholes; he seemed to know that the chocolate was in the circle or in the line and not in some other part of the environment, but the specific location in a particular element of these configurations was not usually perceived. In the case of the square, however, with three boxes on a side, the corners and the middle of each side (the box between two corners) seemed clearly distinguishable to him.

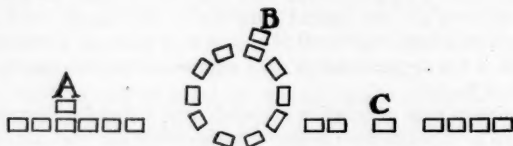


FIG. 1. ARRANGEMENT OF THE MATCH BOXES
Boxes A, B, and C containing the chocolate were picked on the first choice.

When the box containing the chocolate was just outside the line or the circle, *D* made four right and no wrong first choices. Likewise when the chocolate box was face-side up (design showing) in the line, or had a 2-in. space on either side of it, he went directly to it (three trials) without first picking up any of the other boxes (see Fig. 1). He could pick out correctly one specific box, even though it touched another box, if it was not included in the pattern forming the line or the circle.

When 11 boxes were scattered in a disorderly arrangement on the floor in close proximity to one another, *D* was again unable to pick out the box containing the chocolate correctly on the first choice (three trials). He approached the bunch of boxes and then seemed to pick one at random.

Though *D* was probably a typical child, as far as the functions tested were concerned, just as Hertz's two jays were probably typical jays, it was desirable to make more certain of this by repeating the experiment using many children. Several possible sources of error were present in this preliminary experiment and could be avoided. Possibly *D* had not been sufficiently motivated to make the first choice a correct one since he usually got the chocolate eventually anyhow; perhaps the lines between the floor boards were guides and were partly responsible for the few correct first choices made; perhaps the *E*'s personal equation had something to do with the results.

bodily orientation upon the young child's memory for the position of objects, *Child Develop.*, 2, 1931, 125-142.

MAIN EXPERIMENTS

Experiments, similar to those performed with *D*, were carried out by several experimenters at the Antioch Nursery School. The boxes were placed on a uniform background (wrapping paper) and the children were permitted to eat the chocolate only if they picked the correct box on their first choice.⁴

Experimenters *L* and *M* used 11 children for three sessions; experimenters *L* and *S* used 10 children during one session. The children were from 1½ to 5½ yr. old. They were taken singly to a small room; after rapport had been well established the child was shown one of the small boxes (usually little, round pill boxes instead of the match boxes) and encouraged to practice opening and shutting it until he could do so easily. He was given a small piece of chocolate to eat. While the *E*

TABLE I
NUMBER OF RIGHT AND WRONG FIRST CHOICES

Arrangement	Line	Circle	Square	Triangle	Haphazard
No. boxes	6 to 21	12	36	12 or 18	12
No. Ss	21	9	17	13	4
Right	70	4	44	36	2
Wrong	103	5	42	42	2

arranged the boxes (from 6 to 21 in number) in a pattern—a line, a circle, a square, or a triangle—on a large sheet of paper on the floor, the child was seated in a chair about 10 ft. away. He was shown a bit of chocolate as it was placed in one of the boxes and told that he could have it, if after a minute he could pick up the box containing the chocolate without touching any of the other boxes. A screen was placed for one minute between the child and the pattern while his attention was directed to the hand of a stop-watch as it made one revolution.⁵ If on several trials the child failed to find the chocolate on his first choice, to prevent discouragement, it was placed on the next trial where it could be found with relative ease, as in the box at one of the ends of a line of boxes. Not all the children in each group were used for each pattern.

TABLE II
NUMBER OF RIGHT AND WRONG FIRST CHOICES
Chocolate in box

	Just outside line (12 Ss)	Just outside square (2 Ss)
Right	10	2
Wrong	2	0

Results. As indicated in Table I, the children made about as many wrong as correct first choices with all the patterns except the line; in that case, the wrong

⁴ The writer is grateful to Dr. Jane Cape and Dr. Virginia Nelson for permission to use the children at the Antioch Nursery School and to Augusta McMurray, Dorothy Lunt, and Gladys Sterick who acted as experimenters.

⁵ When the choice to be made is one out of three objects, Skalet found that children from 2 to 4 yr. old could make correct selections even after several days delay. (M. Skalet, The significance of delayed reactions in young children, *Comp. Psychol. Monog.*, 7, 1931, (no. 34), 1-82.)

first choices greatly exceeded the correct ones. When the chocolate was placed in a box just outside, but touching, the line or the square the first choices were almost always correct (Table II).

DISCUSSION

Though not a single one of the 21 children could react dependably to the various individual boxes composing the line, the circle, the square, or the triangle, most of them reacted correctly to a much greater extent than might be expected on the basis of merely chance successes. It was apparent both from the children's movements and their remarks that these configurations were not completely unanalyzed wholes. The line had ends and a middle; the square and the triangle included lines having ends and middles. When the chocolate was concealed in a box at one of these points, especially at the ends of a line, it was usually secured without hesitation on the first choice. When the chocolate was not at one of the positions described, the child seemed usually to know at least the general direction in which to look, as for instance, near the middle or on the right. The successes in the case of the circle were due mainly to the fact that the chocolate was concealed every time in the box directly opposite and furthest from the child.

In one session in which 9 children were given a total of 35 trials with the line, of the 12 correct first choices, 11 were when the chocolate was hidden at one of the two ends. Occasionally a child would plead with *E* to "please put the chocolate at one of the ends." The position next to a well recognized one, also yielded a relatively high number of correct first choices. Thus, when the chocolate was in a box next to the end one, it was usually secured on the first choice, but not as frequently as when it was put in the end box itself. Though all the children were asked after each trial how they made their choice, only one child mentioned counting: he said the chocolate was in "the box next to the three at the end."

We had too few children at each age-level to draw any conclusions regarding the relationship of successful first choices to age.⁸ There was also a tendency on *E*'s part to include a smaller number of boxes in the configurations used with the younger children than in those used with the older ones, and possibly to place the chocolate in easier positions for the younger children so they would not become discouraged.

We conclude that though these children between 1½-5½ yr. of age did not react discriminately to each of all the units composing the lines, circles, squares, and triangles with which we experimented, it was possible for them sometimes to single out various aspects of those configurations such as the center, the right, or the left, and even certain specific elements, such as the end or the corner box, and react specifically to them. The configurations were not always completely unanalyzed wholes. The number of correct first choices seemed to depend upon several variables: the number of elements in the configuration, the type of configuration, the particular child, and especially the particular place in the configuration where the chocolate was concealed.⁹

⁸ In the investigation by Emerson (*op. cit.*), the accuracy of selection was positively correlated with age.

⁹ *E* secured almost 100% wrong first choices on the circle and the line in the preliminary experiment with *D* probably because the chocolate was not usually concealed in any one of the distinctive positions in those configurations. For instance, it was very rarely placed at the ends, or even next to the ends of the line.

THE RECOGNITION VALUE OF THE STEPS OF THE DIATONIC SCALE

By HENRY WUNDERLICH, University of Texas

The notes or steps of the major diatonic scale may be designated by the familiar names *do*, *re*, *mi*, *fa*, *sol*, *la*, *si*. The object of the present experiment was to study the relative recognition values of these seven scale notes. The equally tempered diatonic scale was used.

A scale note, like *mi* or *sol*, is not a tone of a fixed frequency but a pitch that has a definite relation to the key-note. As a stimulus it exists, therefore, only in the tonal context that defines the key. The behavior of tones in a melodic series is governed by a number of laws, and, if these laws are broken, the tones lose their character as scale notes and the series of tones loses its established key.

The stimuli used in this experiment were melodic fragments, seven notes long. They are called melodic fragments because each note in the series obeys the melodic laws that govern the scale-step that the note represents. In any such series, the seven notes would mutually define one another as scale-notes, the first three or four notes bearing nearly all the burden of establishing the key. In order to keep this task entirely outside the series proper, each stimulus was preceded by a perfect cadence, which completely established the key.

Each scale-note was used once in each tune. All the notes in a tune lay within the compass of an octave. A tune might, for example, involve the seven scale steps as follows: *si*, *do*, *mi*, *re*, *sol*, *la*, *fa*; with *mi* occupying the highest pitch position and *fa* the lowest. The tune might be pitched at any absolute level that is within a reasonable vocal compass. The procedure was to establish the key first with a perfect cadence (the dominant and tonic chords), then play the tune, pause, and then repeat the tune with one of the notes changed half a step toward the next higher or lower scale note. S was instructed to write down the number of the note which he thought had been changed, whether it was the first, second, third, etc.

In the tune any one of the scale notes could be crucial, *i.e.*, changed on the repetition of the tune. Let us suppose that it is the second scale note, *re*, that is crucial. Then S should answer with the number "4," because it is the fourth note in the series that was changed. A system of numbering was devised so that the tunes could be catalogued according to their use. The tune we are discussing now would be catalogued as 24, because the second note of the scale, occurring as the fourth note in the tune, is the crucial note.

A test consisted of 49 tunes. This number was chosen so that each note could appear as crucial in each of the seven serial positions. After every seven problems S was allowed to rest a little. In each of these subgroups of seven tunes each note appeared but once. All other matters of arrangement were settled by chance, so that no principle of order could be discovered by S. The crucial note could have any rank anywhere in pitch with reference to the other six notes in the tune. Every note occurred once as crucial in each of the seven pitch-ranks in the course of any one

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test. The relation of pitch-rank and serial rank of the crucial note was settled by chance in each test, but, since seven tests were used, it was possible to exhaust all 49 possibilities for each scale step. The absolute pitch of the tune was also determined by chance, but no tune was allowed to go below the *G* an octave and a fourth below middle *C*, or above the *G* an octave and a fifth above middle *C*. In other words, no tune went below the easy compass of a bass voice or above that of a soprano voice. This method of deciding the absolute pitch of the tune by chance resulted in the use of all 12 major keys at random during the test.

ADMINISTRATION OF THE TESTS

The tests were given to undergraduate classes in psychology, which varied in size from 30 to 60 students. The administration of a test lasted about 40 min. Every *S* received a blank having seven rows of seven spaces each. The method of taking the test was first explained carefully, and then a practice problem was played and repeated until all the *Ss* could hear the change. They were then required to record the number representing the sequential position of the note changed. These numbers were entered in the spaces from left to right so that when seven problems had been played, each blank in the first row contained an answer. At the end of every row, a short rest was taken, during which the *Ss* might ask questions. No omissions were allowed. The *Ss* were required to guess when they did not know the proper answer.

Immediately before each pair of tunes two chords were played. These chords served a double purpose. In the first place, they were a warning that the problem was to be presented and *S* was thereby prepared for the beginning of the problem. In the second place, the chords, constituting a perfect cadence in the key of the melody, served to establish its tonality. The second of these two functions was essential. Unless the key is established before the problem is played, the first few notes of the standard have to bear the burden of establishing tonality.

If the previous problem has been in another key, then the first few notes of the new problem may easily be misunderstood as members of the other key. The warning chords, however, were sufficient to destroy the perseveration of the previous key and made it possible for every note to appear as a member of the intended key. Since the rigid specifications under which the tunes were written sometimes interfered with their conforming to melodic laws to the fullest degree, a firm establishment of the key prevented any ambiguity that might result from slightly implausible melodic sequences.

The tests were played on a harmonium, with a single eight-foot stop drawn. This instrument was preferable to a piano because, when the bellows are kept full, it gives tones of equal loudness and there is no chance for *E* to accent any of the notes. A metronome, set at 80 beats per minute, was used as a guide. The tests followed the metronome one note to a beat, with a short space occurring between the warning signal and the standard, and also between the standard and the variant. When the *Ss* were asked for comments, a few of them complained that the metronome was a distraction, but most of them said that it was not. At any rate it is probable that the metronome did not affect the results since it was a constant factor throughout the experiment. There were, however, other distracting factors which probably did affect the results. The tests were given in a room with a high ceiling and plaster walls. It was near the street and there was a great deal of traffic noise. Coughing and other disturbances in the room probably reverberated. It may be that distractions

like these disturbed the regularity of the results. The erratic behavior of one or two problems in otherwise reliable tests might easily be a consequence of such distractions.

RESULTS

Every test had 49 problems. Every note was changed seven times, once in each of seven problems. The noticeability of the change can be measured by the number of Ss who answered the problem correctly. Every problem received a score which is the number of people in the group who answered correctly. For example, Problem 52 might get a score of 19 on the first test, meaning that *sol* is the note that is varied, that it occurs as the second note in the tune, and that 19 people in the group tested noticed a change in the second note and answered 2 in the proper blank on their test sheet. In the second test, Problem 52 might get a score of 28, if more people noticed the change. These raw scores cannot be compared directly. It may be that the second group is superior; or it may be that the pitch-rank of the crucial note is more advantageous; or it may be that the relation of the pitch-rank and the serial position makes the crucial note more prominent. It is the relation of the score to the other 48 scores for a specific test of a specific group that matters. This relation can be expressed by a number that is the quotient of the raw score's deviation from the mean, divided by the standard deviation. This new number is called the x/σ -score. The recognition value of each scale step was measured 49 times in the course of the seven tests, so that each scale step received 49 of the x/σ -scores. It is possible to take an average of these scores because they all represent the degree of difficulty of the change measured in a way that shows the comparative difficulty under the same conditions. Given below is a list of the average x/σ -score for each scale step, and with it is given the standard deviation of the distribution of the 49 scores.

<i>Do</i>	Mean = .25 ± .08	S.D. = .84
<i>Re</i>	Mean = .27 ± .08	S.D. = .83
<i>Mi</i>	Mean = .19 ± .08	S.D. = .80
<i>Fa</i>	Mean = -.42 ± .09	S.D. = .97
<i>Sol</i>	Mean = -.19 ± .10	S.D. = 1.08
<i>La</i>	Mean = .27 ± .08	S.D. = .88
<i>Si</i>	Mean = -.33 ± .08	S.D. = .88

An examination of the distributions of the x/σ -scores for the seven notes shows that the average x/σ -score in the case of *do*, *re*, *mi*, and *la* is better than the general average for all 343 problems. In fact, the distributions for *do*, *re*, and *la* practically coincide, and that of *mi* deviates very slightly. The distribution for *sol* has a mean that is a little below the general average, and a standard deviation that is larger than that for any of the other six distributions. The distribution for this note shows a tendency toward bimodality. A positive value for x/σ -score means that the problem was easier than average, while a negative one means that the change was difficult to detect. The means for *fa* and *si* fall definitely below the general average. The pronounced negative tendency of *fa* and *si* is the most striking feature of the whole array of data.

Of course, there is much overlapping of distributions, even between the highest and the lowest. This was to be expected. Even a tune of only seven notes is a complex of many factors, of which scale step is only one, probably one of the weaker ones from the standpoint of recognition value. Tunes will vary greatly in difficulty,

and hence the scores of the notes will also vary. The other factors should tend to cancel out in a large number of tunes and allow the influence of scale step to be shown in the central tendency of each distribution.

The fact that changes in *fa* and *si* tend to be difficult to detect is probably related to the function of these changes in modulation. *Si*, whenever changed, was lowered a semitone, resulting in a modulation to the subdominant key, a modulation which is very frequent. *Fa*, whenever changed, was raised a semitone, resulting in a modulation to the dominant key, also very frequent. Since these two modulations are to the two most closely related keys, we should expect such a change easily to pass unnoticed.

The bimodality of the distribution for *sol* suggests a double function for that interval. One possible explanation lies in the dual rôle of this step as a member of both tonic and dominant harmony. More extensive investigation is needed at this point.

CONCLUSIONS

Under the conditions of the present experiment, the seven notes of the diatonic scale were found to differ reliably in their average recognition value. The notes that are changed for the simplest modulations are the ones that have the lowest recognition value, *i.e.* *fa* and *si*. *Do*, *re* and *la* have the highest recognition value, whereas *mi* and *sol* are closer to average.

Recognition value is defined by the conditions of the experiment. Only the major mode in equal temperament was used and the change in the crucial note was always a change of a semitone to a note outside the key. Perhaps the method of substituting some other note within the key, or the use of melodic fragments in the minor mode would give different results. The present experiment is a first step in the study of the psychological properties of the scale and its parts, and also in the study of melodic structure.

PREFERRED REGIONS IN MUSICAL COMPOSITIONS AND THE EFFECT OF REPETITION UPON THEM

By HELEN K. MULL, Sweet Briar College

The purpose of this study is, first, to discover how much agreement exists among a group of 'musical' individuals as to which parts of a musical composition are especially pleasing, and, secondly, to note some of the characteristics of these passages which might account for their 'popularity.' Much has already been discovered about the sources of musical enjoyment through the work of Washburn and Dickinson, Gatewood and others.¹ The present study limits itself in this regard to a further examination of one of the known sources of pleasure, the factor of design, the characteristics of the popular passages which relate to their place in the composition as a whole. A third part of the study furnishes certain facts of interest concerning the way in which pleasure grows as the composition becomes more familiar.

The Os were 30 undergraduate students, 20 of them from the Eastman School of Music,² and 10 from Sweet Briar College recommended by the Department of Music on the basis of musical sensitiveness and experience. The compositions selected for investigation were Bach, Sarabande from the Second English Suite for Piano; Chopin, Mazurka, Op. 50, No. 3; and Brahms, Intermezzo and Romanze, Op. 118, Nos. 4 and 5. They were presented by a victrola. Piano compositions were chosen in order to eliminate pleasure based on instrument preference alone; and unfamiliar ones were selected to eliminate differences in familiarity. None of the 30 students had played any of the numbers, and only two remembered having heard any of them before.

The method has the virtue of allowing immediate judgments, made upon material in its context. The Os were given the following instructions:

I shall play you a record, and I should like you to listen to it with the purpose of getting esthetic pleasure *directly* from it. Music, as you know, arouses our feelings, but it may do so in more ways than one. It may do so directly, because of intrinsic qualities, or, on the other hand, indirectly, because of personal associations. It is the *direct* esthetic response that I want. Listen appreciatively but discriminatingly. Whenever you find a passage you think especially beautiful, raise your hand and keep it up until the beauty begins to diminish. In other words, indicate what you consider 'high spots' in the composition. Be as prompt as you can in making these signals. I shall play each record three times, and each time you are to indicate what you consider to be 'high spots.' There is no obligation to indicate the same place each time, nor is there any *a priori* reason for not doing so. Just indicate what you honestly like especially well each time the composition is played.

A stop watch was started with the first note of the record and the periods of preference were noted. These could then be matched with like regions on the score,

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¹M. Schoen, E. L. Gatewood, M. F. Washburn, G. L. Dickinson et al., *The Effects of Music*, 1927, 121-130.

²I wish to express my thanks to Dr. C. W. Fox of the Eastman School for making available both laboratory and observers.

which had been previously marked in terms of seconds to correspond to the record. The purpose of playing each record three times was actually, though the *O*s were kept in ignorance of it, to enable a selection of high spots on the basis of their common occurrence in all three hearings. To determine high spots on the evidence furnished by three hearings is, of course, arbitrary, but the first hearing was included for its freshness, and the third ended the series so as to avoid fatigue and affective adaptation.

RESULTS

Are there, then, any regions in these compositions which qualify as 'high spots' for all *O*s? No region was universally chosen, but some regions rise decidedly above the average of 'popularity' for the whole composition. In all three compositions there are 14 regions found especially pleasant by 15 or more of the 30 *O*s. Here follows a brief analysis of these regions, counting, as the beginning of each region, that place just before its peak of popularity where the average of popularity of the composition is first exceeded, and, as the end of the region, that place after its peak where popularity falls again to average level.

BACH: The first region considered to be a high spot by 15 or more *O*s (by 18 at its peak of popularity) extends from measure 13 to 17. In these measures there is a return to the first part of the first theme, with considerable ornamentation.

The second such region (peak at 15 cases) extends from measure 21 to 24, and is a return of a later part of the first theme, ornamented.

The third region (peak at 20 cases) extending from measure 29 to 33 is a section leading to the most stressed cadence in the whole composition, regarded musically as a climax.

The fourth region (peak at 15 cases), measures 44 to 49, is a repetition of the third region, elaborated.

CHOPIN: The first region (peak at 20 cases), measures 9 to 19, may be described as contrasting with foregoing material by being more definite melodically and rhythmically, and therefore simpler to comprehend.

The second region (peak at 23 cases), measures 46 to 67, is well introduced by a few anticipatory measures, and involves a sophisticated and somewhat surprising change of key, together with a change of mood. It is a bright, simple theme, coming after somewhat diffuse material of indefinite mood.

The third region (peak at 19 cases), measures 78 to 91, is a very slightly ornamented repetition of the second region.

The fourth region (peak at 15 cases), measures 139 to 144, is the same as the first region, elaborated.

The fifth region (15 cases) is just an instant near the end, measure 192, where there is a reminiscence in the tonic of former material in the dominant.

BRAHMS INTERMEZZO: The first region (peak at 18 cases), measures 53 to 71, is new material, simple to grasp, contrasting with antecedent material in mode (major) and key.

The second region (peak at 15 cases), measure 92 to end of composition, is a repetition of the first theme, highly elaborated and intensified.

BRAHMS ROMANZE: The first region (peak at 17 cases), measures 10 to 13, is a repetition of the first theme, with changes.

The second region (peak at 20 cases), measures 18 to 20, is the second theme,

contrasting with the first in key and mood. It is marked *allegretto grazioso*, in contrast to the first which is marked *andante*.

The third region (peak at 18 cases), measures 50 to end of composition, is the second repetition of the first theme, with changes at the end.

Summing up these results, it appears that there in all are 14 regions especially liked by half or more of the *Os*. Nine of these 14 are cases of repetition of thematic material, with changes. Of these nine repetitive regions, five are repetitions of themes not considered especially pleasant when first heard; and four of them are repetitions of themes found highly pleasant the first time they occurred. These four themes, found especially pleasant on first hearing, show features of contrast with preceding material; one of them is merely simpler to grasp, while the other three show contrasts in key and mood as well. The mood of these three regions is in all cases brighter than the antecedent mood. Finally, one of the total 14 cases is a stressed cadence, a marked point of arrival, in other words, in this case a musical climax.

The data of the experiment also furnish facts concerning the manner in which pleasure increases with repeated hearings of the composition as a whole. Considering the first and third hearings, we find the following increase in average length of high spots: Bach, from 16 to 21 sec.; Chopin, from 23 to 25 sec.; Brahms (Intermezzo) from 22 to 32 sec. and (Romanze) from 25 to 30 sec. The average increase for all compositions is from 20 to 29 sec., the longer period in the third hearing being true for 24 of the 30 *Os*. Of the other six *Os*, three show equal periods in both hearings; two, a decrease of 2 sec. in the third hearing; and one, a decrease of 1 sec.

In a great many cases the greater extent of the high spots in the third hearing appears as a *spread about the initial focus*. (By initial focus is meant the corresponding high spot in the first hearing.) Altogether there are 300 such cases out of a total of 468 high spots in the third hearing, while only 52 new, isolated high spots appear. (The remaining 116 cases spread about foci in the second hearing, many of which themselves extend from foci in the first hearing.) A spread in both the anterior and posterior direction from the initial focus is the most common case. Also, anterior spread is more frequent than posterior. Following is a diagram of all the relations between corresponding high spots of the first and third hearings, together with the frequency of their occurrence.

1.	====	Bidirectional expansion	123 cases
2.	====	Anterior expansion, posterior contraction	102 cases
3.	====	Anterior contraction, posterior expansion	61 cases
4.	====	Anterior expansion, posterior equality	58 cases
5.	====	Bidirectional contraction	42 cases
6.	====	Anterior equality, posterior expansion	33 cases
7.	====	Anterior equality, posterior contraction	29 cases
8.	====	Anterior contraction, posterior equality	21 cases
9.	====	Equality	20 cases

From these figures it may be seen that the total number of cases of anterior expansion is 283 (items 1, 2 and 4); of posterior expansion, 217 (items 1, 3 and 6); of posterior contraction, 173 (items 2, 5 and 7); and of anterior contraction, 124 (items 3, 5 and 8).

There are a number of points here about which it is interesting to conjecture. First, why does the increase of pleasure in the third hearing of the composition appear very largely as spread about a focus? And second, why does anterior spread preponderate over posterior? Some of the spread may be due simply to the abandonment, with familiarity, of an initial cautious attitude, and the consequent greater readiness to acknowledge a region as highly pleasant. But this would not explain the greater frequency of the anterior spread, unless, indeed, one supposed that once the hand was up in the first hearing, there was caution also about lowering it, so that on the third trial when caution had been abandoned, the posterior spread did not show for so much. This supposition, however, does not take into consideration why so few new, isolated high spots appear in the third hearing. Another explanation, which does take this fact into account, suggests itself. Might it not be that learning to like a composition has features similar to those involved in an animal's learning to run a maze—that is to say, in the case of our experiment, an original high spot (corresponding to the maze goal) may be thereafter anticipated and a pleasure gradient extend backward? Thus, pleasure would actually spread from a focus, rather than appear *de novo*. Once the climax is reached, there would be relaxation of interest and some tendency for the pleasure to drop off. This would explain the relatively large number of posterior contractions. Posterior expansion, where it does occur, might be due to familiarity, or to hang-over from the climax.

CONCLUSIONS

(1) There is no region in any of the compositions studied especially liked by all *O*s, even though the *O*s belong to a fairly homogeneous musical group. The most popular region found involved 23 of the 30 *O*s.

(2) Counting as popular those regions especially liked by half or more of the *O*s, it is found that there was a total of 14 such regions, nine of which were repetitions, with changes, of previous themes. Four of the 14 contrasted with antecedent material, one chiefly in degree of simplicity (preferred region simpler), and three others, not only in the greater simplicity of the preferred part, but in greater brightness of mood, and in change of key. One region culminated in a highly stressed cadence, regarded musically as a climax.

(3) Regions of special pleasure are of longer duration in the third hearing of each composition than in the first.

(4) The greater duration of high spots in the third hearing appears as a spread about the initial focus.

(5) This spread is most frequently found at the anterior end of the focus.

(6) The explanation of this anterior characteristic of the spread may be that it is a case of a goal gradient, in which high spots are goals which extend their influence backward.*

* M. F. Washburn, *The Animal Mind*, 1936, 373-381.

THE DEPENDENCE OF APPARENT VISUAL SIZE UPON ILLUMINATION

By ALFRED H. HOLWAY and EDWIN G. BORING, Harvard University

In the course of certain other experiments on the relation of apparent visual size to the distance of the perceived object¹ and on the apparent size of the moon,² we undertook to determine the dependence of apparent size upon illumination. We report the result here since we are not in a position at present to extend the investigation to a greater number of illuminations, distances, and *O*s.

O was seated at the right-angle junction of two long corridors in the Harvard Biological Laboratories. He sat so that he could look down either corridor by turning his head 45° to one side or the other. The experiments were made after midnight in corridors entirely dark except for the faint light furnished by the two stimuli.

The standard stimulus, placed remotely from *O* in the corridor at his right, was a projected circle of light, 53 cm. in diam., with an intensity of 2.4 ml. at a distance of 92 cm. This is the intensity for the rows labelled "a" in Table I, where the "relative illumination" is called 1.00. By introducing filters into the projection beam we could reduce this intensity to 0.10 and 0.01 of its original intensity ("b" and "c" in the table). The standard was first placed at 30.5 m. from *O*, a distance at which its diameter subtends an angle of 1.0°. Later the distance was doubled so that the subtended angle was 0.5°.

The comparison stimulus was placed in the corridor to *O*'s left at the same distance from *O* as the standard stimulus. It was a projected circle of light, of variable size and constant intensity. By altering electrical resistance in the circuit of the projector, the comparison intensity was equated to the intensity of the standard without filters. Its size was varied by running through the projector a strip of film on which was a series of 50 apertures, for the most part differing from each other in successive frames by just not noticeable differences.³

O looked back and forth between the standard and comparison stimuli. He called to *E* to adjust the size of the comparison stimulus until he was satisfied that the two stimuli were the same apparent size. Then *E* measured the actual size of the comparison stimulus. Since the two stimuli were equidistant, little difficulty was encountered in equating apparent sizes of the objects.

The results appear in Table I. Row "a" in both halves of the table and with all 3 *O*s shows that there was a small constant error in favor of the comparison stimulus, which, with distance and illumination both constant, was judged slightly larger than the standard stimulus (1.15, 1.05 and 1.02 for 1.00°; 0.55, 0.51 and 0.52 for 0.50°).

* Accepted for publication March 30, 1940.

¹ A. H. Holway and E. G. Boring, Determinants of apparent visual size with distance variant, to be published later in this JOURNAL.

² Holway and Boring, The moon illusion and the angle of regard, this JOURNAL, 53, 1940, 109-116; and more especially the latter paper by us. The apparent size of the moon as a function of the angle of regard: further experiments, *supra*, 537-553.

³ Holway and Boring, *op. cit.*, 110 f.

Each half of the table shows that dimming the standard stimulus decreases its apparent size, and that the decrease is greater for greater dimming. At a distance of 30.5 m. an illumination-ratio of 10 gives on the average a size-ratio of 1.12, and an illumination-ratio of 100, a size-ratio of 1.38. The corresponding average size-ratios for the stimuli at a distance of 61 m. are 1.15 and 1.60.

Schur found that the variation in the apparent size of artificial moons depends upon the distance of the moons from O , that there is very little variation at 3 m.,

TABLE I
DECREASE IN APPARENT VISUAL SIZE WITH DECREASE IN ILLUMINATION

θ = size (degrees) of comparison stimulus which matches standard = apparent size of the standard. Each θ is average of 5 observations. σ = standard deviation of distribution of θ s. Illumination of comparison stimulus (and of standard stimulus = 1.0) is 2.4 ml. at 92 cm. The ratios, θ_a/θ_b and θ_a/θ_c , show the amount of change in the apparent diameter of the disk due to change of illumination.

Standard stimulus			E.G.B.		A.H.H.		L.M.H.	
Size and distance	Rel. illum.		θ	σ	θ	σ	θ	σ
1.0° at 30.5 m.	1.00	a	1.15	.045	1.05	.038	1.02	.032
	0.10	b	1.06	.032	.90	.021	.93	.026
	0.01	c	.81	.032	.79	.028	.75	.029
		θ_a/θ_b		1.08		1.17		1.10
		θ_a/θ_c		1.42		1.35		1.36
0.5° at 61.0 m.	1.0	a	.55	.021	.51	.015	.52	.011
	0.10	b	.51	.043	.42	.025	.45	.015
	0.01	c	.32	.011	.35	.026	.32	.011
		θ_a/θ_b		1.08		1.21		1.15
		θ_a/θ_c		1.72		1.45		1.62

and that maximal variation is reached at about 33 m.⁴ The implication is that some of the determinants of apparent size are inoperative at great distances, so that the effect of other determinants might be more easily demonstrated. For this reason we chose at first to work at 30 m., expecting to find even greater differences in apparent size than we did find. Later we doubled the distance, but we are not prepared to assert on this evidence that the size-intensity phenomenon is more pronounced at 60 m. than at 30. It is true that no ratios at the longer distance are less than the corresponding ratios for the shorter distance, and the ratios for the longer distance are considerably larger for the illumination-ratio 100 than for the illumination-ratio 10. Nevertheless knowledge of the exact form of the general function, and in particular the answer to the question as to whether the effect diminishes at distances much less than 30 m., must depend upon further research.

We undertook this experiment originally in hopes that its results would throw light upon the nature of the moon illusion. The size-ratios for illumination-ratio 100 at 60 m. are of the order of the size-ratios in the moon illusion for 0° and 30°

⁴ E. Schur, Mondtäuschung und Sehgrösskonstanz, *Psychol. Forsch.*, 7, 1925, 44-80, esp. 53-57.

elevation of the eyes.⁸ At present, however, it is not clear to us how raising the eyes 30° could diminish illumination at the retina to one per cent of the value that it has when the eyes are in the primary position.⁹

⁸ Our second paper (*supra*, 537-553) shows that the moon illusion is dependent only upon movements of the eyes and not upon movements of the neck and body, and that most of it occurs, therefore, when the head is fixed, in the first 30° of elevation of the eyes.

⁹ A casual demonstration of the effect of intensity upon apparent size can be made by viewing the full moon binocularly and monocularly. Most persons will agree that the diameter of the moon in monocular vision is about 0.9 its diameter in binocular vision, and certain unpublished experiments by us show that the monocular reduction may be as great as 0.85 or even 0.8.

PHANTOM LIMBS

By S. FELDMAN, Cornell University

So large was the number of limb-amputations in the World War—it was estimated at 60,000—that psychologists were called upon to help deal with the many problems growing out of this mass-mutilation.¹ These problems entailed, among other things, a study of the phantom or hallucinated limb that appears after amputation. For, strange as it may seem, this hallucination functions in a practical way. By inducing an apparent movement of the phantom arm the subject is able to innervate the biceps and triceps muscles remaining in the stump.² The phantom functions also in the control and appreciation of the movements of the artificial limb. At first unrelated, the two come together, arrive at spatial coincidence, and the lifeless appendage is animated by the living phantom.³

What is the basis of the phantom? Does it perhaps rest upon a confusion with the other, unamputated limb? No; for in cases of double amputation, where there is no possibility of confusion, the phantom still appears—a double phantom.⁴ Nor does it suffice to refer to the fact that the parts of the body are represented in the brain in topographical detail, since this topographical representation is only one factor in the perception of the body and its parts, present or missing. In the case of missing parts of the body, we still do not know how and where perception is initiated.

I suggest (1) the paradox that it is the absence of the limb that is the cause of its perception, and (2) that the perception of the phantom is initiated by movements of the body—any movement of any part of the body may be responsible.

(1) *The paradox.* As a rule, we are not conspicuously aware of the several parts of the body. These generally obtrude themselves only when something goes wrong or something out of the ordinary happens to them. The stiff limb, the numb limb, calls attention to itself; the normal member does not. Just so it is the absent limb that makes a show of itself, not the limb that is still present. It is characteristic of the phantom limb that it appears of its own accord, spontaneously and involuntarily, and recurs so frequently as to cause annoyance to the subject; it haunts him. This insistence of the absent limb contrasts markedly with the inconspicuousness of its normal mate.⁵

The limbs behave, in this respect, as familiar objects generally do. The more permanent fixtures in one's surroundings—say, a picture hanging in its accustomed place on the wall—receive little or no attention until they are missing. An analogy

* Accepted for publication May 16, 1940.

¹ The best psychological account, a study of more than a 100 Ss, is by David Katz. (1) *Psychologische Versuche mit Amputierten*, *Zsch. f. Psychol.*, 85, 1920, 83-117. (2) *Zur Psychologie des Amputierten und seiner Prothese*, *Zsch. f. ange. Psychol.*, Beiheft 25, 1921, 1-118. (3) *Psychologische Erfahrungen an Amputierten*, *Bericht ü. d. VII Kongress f. exper. Psychol.*, 1922, 49-75 (includes a bibliography of 58 items). All further footnote references to Katz in this article are to (2).

² Katz, 49 f.

³ Katz, 24.

⁴ Katz., 42 f.

⁵ Katz, 15.

that is, in some ways, even closer is the after-image of touch following the removal of one's hat or spectacles. A person may search for his spectacles while they are resting—unfelt—on the nose; the object is there, but it is not perceived. At another time he may find himself reaching up for his glasses after he has just taken them off; so insistent can the after-image illusion be.⁶

(2) *The kinetics of the limb.* How is the normal limb perceived? Light on this question comes from an experiment on the perception of lifted rods. This is not surprising, since an object held in the hand functions as an extension of the limb itself.

How, then, do we perceive an object extending out from the hand? How, for example, does the fisherman feel the fishing rod? In the experiment, an *amputated rod*—that is to say, a mere handle—was placed in the subject's grasp. Then, by applying to the handle a down pull at one end and an up pull at the other, it was found possible to create a *phantom rod*, the illusion of a rod extending beyond the hand.⁷ This experimental achievement demonstrates that the perception of the rod depends upon the rod's lever action, or rather upon the adjustment of body, arm, and hand to the lever action of the rod.

Now a limb is a balanced system of weights and levers. The upper arm, for example, is suspended from the shoulder, the forearm from the upper arm and also from the shoulder, the hand and fingers from all three. When an object is held in the hand, another element is added to the system. What has been shown to hold for the rod must, therefore, also hold for the limb: the perception of a limb depends upon the body's adjustment to the limb's lever action. If a limb continues to be perceived after it has been amputated, it means that an essential condition of its perception has survived: the body continues to adjust itself as if the lever actions formerly exerted by the arm and its several parts were still present. Amputation does not change the body's habitual adjustment.

For the direct initiation of the phantom limb we must hold movement primarily responsible. Whenever one moves any part of the body, a readjustment takes place involving all the limbs. As long as the readjustment proceeds without a hitch, the limbs remain unperceived. We become aware of them only when a hitch occurs. When an arm has been amputated, almost every movement that is attempted entails a hitch. Important in this connection is the fact that the arms are organs of emotional, and also of intellectual, expression.⁸ We emote and think with our arms, some of us more than others. It is no wonder then that the phantom arm is described as recurring frequently and persistently.⁹

The phantom limb, so it is reported, is subject to change. The hand gradually clenches into a fist. It also gets smaller and moves closer to the stump. In its final stage it is described as a fist of child's size nestling *inside* the stump.¹⁰ These changes

⁶ For a detailed description of this phase of perception see Boring, Langfeld and Weld, *Introduction to Psychology*, 1939, 413 ff.

⁷ L. B. Hoisington, On the non-visual perception of the length of lifted rods, this JOURNAL, 31, 1920, 136 ff.

⁸ Katz (9 f.) deals with the intellectual and emotional disturbances that follow the loss of an arm.

⁹ The fact that stimulation of the stump may affect the phantom is not inconsistent with our analysis.

¹⁰ Katz, 21-24.

are evidence of gradual adjustment to the lacking limb.²¹ The body wakes up but slowly to the fact that a part of it is really missing.²² In one case reported, the phantom was still present after 56 years!²³

²¹ It is significant that the stump suffers a parallel apparent shrinkage (Katz, 58-63). In the rod experiment, different lengths of the illusory rod were obtained by varying lever action. Lever action is basic to the perception of body-parts and of objects in contact with the body, including tools and clothing (Hermann Lotze, *Mikrokosmos*, 2nd ed., 2, 1869, 202-212; H. C. Sanborn, The function of clothing and bodily adornment, this JOURNAL, 38, 1927, 1-20). The student who wears his or her hat at a tilt, the man who pushes his hat back when in an argument, gain thereby a feeling of added height and confidence.

²² Our analysis probably applies also to the phantom limbs reported in cases of thalamic and other brain lesions. See Paul Schuster, Beiträge zur Pathologie des Thalamus Opticus, *Arch. f. Psychiat. u. Nervenkb.*, 105, 1937, 358-622; 106, 1937, 1-233. The effect of a lesion, however, is variable; one has to reckon with progressive deterioration and also with occasional compensation. When the use of a limb is lost gradually, as through freezing or tuberculosis, no phantom appears (Alfred Gallinek, The phantom limb, *Amer. J. Psychiat.*, 96, 1939, 414).

²³ Katz, 17.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY
OF CORNELL UNIVERSITY

LXXXIX. GENERAL PRACTICE IN MIRROR TRACING

By T. A. RYAN and FRANCES SCHEHR

The most important skills which we gain in everyday life are probably those which involve learning a labile mode of performance which readily changes to fit the needs of a new situation. Such skills are rarely learned by a step-wise process of learning first one performance for one specific situation, then another, and finally putting all of these specific 'responses' together into a composite which we call skilled performance. Instead, one learns many different aspects of tennis, writing, or typing all at once.

The present study deals with mirror tracing because it lends itself to the development of such labile performances and because it is a fairly simple and readily observed kind of activity. The general question is, what is learned when one repeatedly traces a given pattern while the visual pattern is inverted by a mirror? How much of the increase in speed is brought about by fixing the specific movements required for a given design, and how much by developing a general skill of coördinating movements with an inverted visual pattern?

The large number of experiments upon transfer of training from one bodily member to another indicate that there is an appreciable amount of this new, general coördination built up.¹ The fact that the transfer may go from hand to foot of the same side indicates that the transfer is not an incidental result of the bilateral symmetry of the body.² Because of the necessity of using the feet in Bray's experiment, however, the task had to be so simplified that it may have especially favored the development of a general "adjustment to the reversing effect of the mirror" which Bray suggests as one of the major determinants of transfer.³ It may be that this adjustment is less evident when an S must trace a more or less complicated design. This might involve almost entirely the development of skills specific to the pattern traced. In such a case there might be less general reversal of movement with respect to the visual pattern.

The development of the same sort of skill involving reversal of customary relations between vision and motor activity is to be found in experiments like that of Stratton in which the visual stimulus is inverted or shifted for all objects over a long period of time.⁴ Here practice is of the general, unsystematic kind typical of every-day learning, and the new adjustment seems to be non-specific. Wooster, in a study involving a small amount of lateral shift of the visual field by means of

* Accepted for publication March 13, 1940.

¹ E. g. P. H. Ewert, Bilateral transfer in mirror-drawing, *Ped. Sem.*, 33, 1926, 235-249.

² C. W. Bray, Transfer of learning, *J. Exper. Psychol.*, 11, 1928, 443-467.

³ *Op. cit.*, 460.

⁴ G. M. Stratton, Vision without inversion of the retinal image, *Psychol. Bull.*, 4, 1897, 341-389, 463-481.

prismatic lenses, found a gradual reduction in errors of localization even without any perception of the accuracy of movements which the subject makes.⁶ She explains this as involving an adjustment to the new position of the eyes in the head in forward orientation. In other words, it is a general change independent of the special tasks set for the subject. Something of the same general kind may be involved in mirror-tracing skills.

A study of more direct bearing upon our present problem is reported by N. Förster.⁶ She was interested in finding the nature of the change in eye-hand coordination in mirror-tracing. Since there is transfer from one design to another in this task, she regards the change as a modification in the general eye-hand coordination, and wishes to show the relative rôles of kinesthesia and vision in this change. Subjects who had practiced the task were questioned about their methods of drawing after skill was developed. They reported that they finally came to 'forget the mirror' and that the performance was as natural as normal tracing. After the training period, the subjects were asked to draw the design from memory. She finds that the drawing is made in a reversed position *with the eyes closed*. This is taken as evidence that direction of movement of the arm was perceived visually rather than kinesthetically during the learning, so that S drew from memory in terms of the visually apprehended pattern even though the arms had to move in the opposite direction from that used during the training experiments. Kinesthetic guidance is therefore supposed to be of relatively slight importance. It would seem from this that the effect of practice was to teach S to ignore kinesthetic cues. This finding is worth checking, but our present problem attacks another aspect of the skill.

Problem. This is a study of the effect of a form of practice deliberately designed to favor the development of a labile and adaptable performance. Our problem was: does practice which consists of a mixture of trials upon several different designs favor the development of a general skill more than specific practice upon a single design? The generality of the skill is measured in terms of the amount of transfer-effect to unpracticed designs.

This formulation of the problem may suggest a resemblance to studies like that of Dashiell on the 'complete' versus the 'alternate' methods of learning two habits.⁷ In learning two different mazes, or in practicing card-sorting according to two different systems the situation is quite different from that involved in learning to draw two or more designs in mirror tracing. It is doubtful if there is any general skill of maze-running or of card-sorting which runs through the two alternative sequences being learned. Certainly there is no general principle of organization between visual perception and motor performance of the sort involved in mirror tracing. The Ss in Dashiell's experiment were learning two conflicting habits or sequences.⁸ Our Ss

⁶ M. Wooster, Certain factors in the development of a new spatial coordination, *Psychol. Monogr.*, 22, 1923, (no. 146), 1-96.

⁶ N. Förster, Die Wechselbeziehung zwischen Gesichts- und Tastsinn bei der Raumwahrnehmung, *Psychol. Forsch.*, 13, 1929, 64-78.

⁷ J. F. Dashiell, A comparison of complete versus alternate methods of learning two habits, *Psychol. Rev.*, 27, 1920, 112-135.

⁸ One pair of tasks used by Dashiell (horizontal adding and vertical adding by alternate numbers, *op. cit.*, 132) might be said to involve both common and conflicting features, so that it is difficult to say what part was played by increase of general adding skill and what by increase in skill in following the two separate orders of procedure.

were learning the same general system of eye-hand coordination applied to slightly different concrete situations.

This experiment grew out of certain observations made in an advanced undergraduate laboratory course at Cornell. Four students who had previously performed a standard experiment in mirror tracing with straight-line figures were given more complicated curved figures to trace. Four different figures were employed and practice was rotated from design to design so that each trial of a particular design was separated by trials of the other three designs.

The amount of improvement in speed of drawing was surprisingly low under these conditions of general or mixed practice. We found that normally, with ordinary uniform practice of a single design, performance was stabilized by the end of thirty trials, and the time was usually halved. With this rotated practice, however, the performance was by no means stabilized after a total of thirty trials, and the reduction in time was relatively small. The following Table I gives the results for the four Ss, in terms of percentage of reduction in time, comparing the first eight and the last eight trials out of a total of thirty trials distributed over four designs:

TABLE I
INCREASE IN SPEED
(Thirty trials, rotated practice)

S	% reduction in time
H	22
I	19
M	14
S	14
Mean	17

In this series of 30 trials, any single design had been practiced, of course, only 7 times, so that if we consider mirror tracing as a process of learning to draw a specific figure, the above results are not surprising. It would appear, therefore, that there is very little general 'learning-to-trace-in-a-mirror' involved here. All of these Ss had, however, performed another mirror tracing experiment a month or two before. Our first problem which arises from these results is, then, to carry out a similar procedure with completely unpracticed Ss and using controls of the more common uniform practice.

In two of the above cases we had introduced a fifth design which was traced before and after the rotated or mixed practice. Records on this fifth design were used as a measure of the transfer effect of this mixed practice on a strange design. The rotated practice was carried further here so that a total of 60 trials, 15 of each of the four designs, came between the two practice periods of the control design. There is a hint here that the total reduction in time for any design is the result of general learning rather than specific skill for a particular design. In other words, the transfer effect shows quite a different picture from that derived from a study of the learning curves themselves. There is as much reduction for the unpracticed design as there is for the practiced patterns. The comparisons of the control and practice designs are found in Table II.

These results are to be regarded as no more than a hint, since the 2 Ss had previously practiced mirror tracing, and since the transfer effect can be computed only

on the basis of single trials before and after practice. The reliability is heightened, however, by the uniform progression of the learning curves in each case.

The second part of our main problem is derived from this hint. We wished to study the transfer effect of rotated practice as well as the learning curves. Control series showing the effect of homogeneous practice upon unpracticed designs are essential, if we are to assess the effectiveness of general practice.

The results to be reported below throw further light upon these two aspects of the problem of general learning in mirror tracing. Unfortunately, we cannot give any conclusive answers as yet. The time-consuming nature of the practice periods made it impossible to secure results on the large number of subjects which would be necessary to make the results completely reliable. Because the study was carried

TABLE II
REDUCTION IN TIME AFTER ROTATED PRACTICE

S	Transfer design	Practice designs				Av.
		1	2	3	4	
H	36%	46%	44%	49%	47%	46.5%
M	46%	34%	26%	25%	29%	28.5%
Mean	41%	40%	35%	37%	38%	37.5%

on by the junior author as an undergraduate research, limitations of time had to be considered. It is hoped that a further study will round out the picture.

Procedure. Six Ss, all unpracticed in mirror tracing, served in the main experiments. Practice periods of a half hour three days a week continued until the required number of trials had been reached.⁹

The tracing apparatus consisted of the usual horizontal drawing board with a vertical mirror in front of S and a screen over the hand. The designs to be traced were made with single lines. They consisted of curved and slanting lines so that almost the whole course of the drawing was more or less oblique with respect to the plane of the mirror.

S was given the following instruction:

Take the pencil. When I say 'go' proceed to trace the figure. Take care to keep on the line until you have completed the figure: your pencil is not to leave the line; if you go off the line, retrace the part you have missed. Try to go as fast as possible, but make sure that you are accurate.

The Ss were divided into two groups. The first group (3 Ss) was given the rotated practice, tracing first one design and then another as described above. Four different practice designs were used. The second group (3 Ss) practiced only one design. All Ss traced the control or transfer design before and after this practice series. Thus we are able to compare the learning curves for rotated and uniform practice, and also the amounts of transfer to a new design resulting from the two kinds of practice. In addition to the 6 Ss of the main groups, two other Ss practiced a single design without any transfer test. These results are used in studying the rate of learning.

⁹ In two instances there were unavoidable interruptions in this schedule, but the method of computing improvement makes these interruptions relatively unimportant.

In the rotated practice series, all Ss were given a total of 100 trials, 25 for each design. At the end of this time, each of the 3 Ss was highly stabilized both in speed and accuracy. The remaining 5 Ss were each given 30 trials of a single figure, and then as many additional trials as were necessary for stabilization of the performance to become evident. Since, however, only slight changes occurred after 30 trials had been reached, only the first 30 will be considered in our study of the rate of learning.

Rate of learning. The comparison of rate of improvement under the two conditions involved is difficult because of the lack of a common standard. We shall therefore make the comparison on two different bases. (1) If improvement is due entirely to the fact that S is learning mirror-tracing-in-general, the rate of improvement should be the same whether the practice all involves the same design or whether it includes trials with a number of varied designs. Therefore the first comparison is in terms of rate of improvement per trial of practice, regarding all trials as equivalent whether the practice is mixed or uniform.

(2) If improvement is attributable entirely to specific skills for a specific pattern traced, mixed practice should be less effective than uniform practice in terms of the total number of trials of practice. In the extreme case, with no elements of skill

TABLE III
RATE OF LEARNING IN THE FIRST 30 TRIALS
(Percentage time-reduction, last eight vs. first eight trials.)

Rotated Practice		Uniform Practice	
S	% reduction	S	% reduction
1	35	4	49
2	40	5	51
3	16	6	44
		7	48
		8	52
Mean	30.3	Mean	48.8

common to the designs in mixed practice, improvement in drawing one design would be completely independent of the amount of practice on other designs interspersed during the training period. Thus 25 trials on a single design should bring as much improvement as 25 trials on the same design which are interspersed among 75 other trials devoted to three other designs. In keeping with this possibility, the records for each design involved in the rotated or mixed practice series were considered separately as isolated learning curves.

Table III gives the analysis suggested by the first of these possibilities—that the practice improves only mirror-drawing-in-general. The table shows the percentage of reduction in time of the last eight out of thirty trials, compared with the first eight.¹⁰ The eight trials grouped together in computing improvement during mixed practice involve, of course, two trials for each of four designs, while the first and last groups for uniform practice consist of eight trials of a single design.

¹⁰ All of the rates of learning are given in terms of reduction of time. Error records were taken, but the unreliability of counting errors involving deviations from a single line is very great. Also, it is certain that errors vary in degree, and the estimation of the degree of error is still more unreliable. Our instructions were so worded that S had to retrace any parts of the line he missed during an error. Therefore the time records reflect a composite of speed of movement plus additions to the total time due to errors.

If these results are borne out on further *Ss*, they indicate that 30 trials distributed over several designs do not result in the same degree of improvement for any one design, as thirty trials concentrated upon the same design.

Indeed our second assumption seems to be more nearly borne out. If we consider the 25 trials for each design in the rotated practice in the same way we would an isolated practice series, there is no more improvement than there is after 25 trials on a single design with no other practice interspersed. Table IV shows the percentage reduction in time in 25 trials, comparing the first two with the last two in each group. The rotated practice was spread out over a longer interval because of the larger number of trials involved and also because of unavoidable interruptions in the

TABLE IV
REDUCTION IN TIME AFTER 25 TRIALS OF EACH DESIGN
(Percentage time-reduction, last 2 vs. first 2 trials.)

Rotated practice		Uniform practice	
S	% reduction	S	% reduction
1	56	4	66
2	63	5	71
3	52	6	46
		7	58
		8	55
Mean	57	Mean	59.2

schedule for *Ss* 2 and 3. But, if anything, this should have increased the effects of rotated practice.

Thus a total of 100 practice trials on four designs results in no more increase in speed for any one design than would result from practicing that design alone for 25 trials. Of course, *Ss* 1, 2 and 3 have learned four designs while the remaining subjects have learned only one, but the four designs required four times as much practice.

Transfer of effect to unpracticed designs. Our preliminary findings had suggested the possibility that rotated practice might result in greater transfer to new designs than uniform practice. In the preliminary experiments we found as much reduction in time for unpracticed designs as for the designs which were actually practiced in rotation. The following results confirm this finding, but indicate that it is not to be attributed to the rotation of practice. Table V shows the percentage of change of time of the transfer design, compared to the percentage change of the practiced designs. In order to make the figures comparable, the reductions are all computed from the first and last trials.

There are a number of implications in these results. First, there is *more* transfer from uniform practice than there is from rotated practice, in spite of the fact that the uniform practice series involved only 30-40 trials altogether, while the rotated practice comprised 100 trials.

In both cases the improvement in the transfer design is greater than the improvement in the practiced designs. This would not be surprising if we regarded the performance as learning mirror-tracing in general. If this is what *S* is doing, the transfer design results could be placed upon the same learning curve as the practiced designs, and, since it comes at earlier and later parts of the curve, we should expect a greater percentage of change. Such an assumption would, however, con-

tradict the earlier analysis of the rate of learning which seemed to show that the learning of various specific designs goes on independently.

Conclusions. While these results are apparently contradictory, it is possible to reconcile them. It must be remembered, however, that this suggestion is only provisional, pending further study designed to test the reliability of the results themselves.

The indication seems to be that anyone interested in developing skill for mirror-tracing in general would find it most profitable to learn one specific pattern of performance well first. The generalization will then take care of itself. In other

TABLE V
TRANSFER EFFECT

Rotated practice			Uniform practice		
S	% reduction		S	% reduction	
	transfer	practiced		transfer	practiced
1	69	61.5	6	71	43
2	59	66	7	78	63
3	74	57	8	82	55
Mean	67.3	61.5	Mean	77	53.7

words, our attempt to develop a general mirror-tracing skill by a special system of rotated practice would seem to have been unprofitable.

The explanation for this may be that shifts from one design to another confuse the issue by preventing the subject from disposing of certain non-essential but troublesome details involved in the specific designs. By repeatedly practicing the same design, peculiarities of the particular design may soon become unimportant so that the later part of the practice is devoted exclusively to mastering the general technique of mirror-tracing. When the designs are changed after each trial, the new special pattern may draw the emphasis away from the general technique. This would account for both the slowness of learning with rotated practice, and also for the fact that rotated practice is less effective in determining transfer than is uniform practice.

Even if further data substantiate these findings, we would still have several problems to be solved. For example, it is possible that other systems of practice involving more than one design might be more effective than uniform practice. In addition, there is the question whether similar relationships can be found for performances other than mirror-tracing.

These results are also significant from the theoretical side. A treatment of transfer in terms of identical elements runs counter to our findings. An individual who has learned four designs has certainly learned a great many more specific connections between perceived direction of line segment and direction of movement to be made. Therefore those who had engaged in rotated practice should, by chance alone, have learned more components of the unpracticed design used in testing transfer than those who had practiced only a single design. Yet the transfer under the two systems of practice is not significantly different.

It would seem much more plausible, therefore, to conceive of the change resulting from practice as involving a reorganization of the total system of relations between visual pattern and direction of movement. This system must be regarded as more or less unitary, rather than as a patchwork of elemental connections.

APPARATUS

AN INEXPENSIVE SLOW FREQUENCY OSCILLATOR FOR DRIVING ELECTROMAGNETIC MARKERS

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In many psychological experiments where either tape recording or photography is used for securing records, some type of device is employed for indicating the time intervals. Many kinds of such devices are available but almost all of them have some disadvantage in actual operation. The Jacquet clock, the Harvard clock, synchronous motor driven commutators and a number of oscillating circuits, each have some practical difficulty. In some cases, it is a question of contacts; in others, a matter of maintaining a constant voltage; and in still others, either a problem of cost or difficulty in securing machine shop facilities for construction. The timer proposed in the following diagram can be constructed from standard parts and can be assembled without any machine work. It is almost noiseless, has a wide range of time intervals, can be used on any 110 v. A.C. circuit, and requires no care once it is assembled. The time intervals are varied by means of fixed resistors R_1 - R_3 , R_a - R_b , R_c - R_d , R_e - R_f , R_g - R_h , R_1 - R_j which can be thrown in or out of the circuit by switch 1 (S_1)—a six-point double pole non-shorting type (see Fig. 1). The following time intervals may be secured by use of the various resistors. The manufacturers ratings of the resistors and condensers may differ by as much as 10%

Explanation of symbols of Wiring Diagram

R_1 - R_3 = 30 meg. Ω , $\frac{1}{2}$ w., IRC	R_7 = 500,000 Ω , $\frac{1}{2}$ w., IRC
R_a - R_b = 10 meg. Ω , $\frac{1}{2}$ w., IRC	R_8 = 500 Ω , 2 w., IRC
R_c - R_d = 5 meg. Ω , $\frac{1}{2}$ w., IRC	R_9 = 50,000 Ω , 10 w., Ohmite
R_e - R_f = 1.25 meg. Ω , $\frac{1}{2}$ w., IRC	R_{10} = 10,000 Ω , 2 w., IRC
R_g - R_h = 0.75 meg. Ω , $\frac{1}{2}$ w., IRC	R_{11} = 10,000 Ω , 2 w., IRC
R_1 - R_j = 0.35 meg. Ω , $\frac{1}{2}$ w., IRC	R_{12} = 10,000 Ω , 1 w., IRC
R_3 = 2,500 Ω , 10 w., Ohmite	R_{13} = 40,000 Ω , 20 w., Ohmite
R_4 = 600 Ω , 10 w., Ohmite	T_1 = R.C.A. 6 J 7
R_5 = 500,000 Ω , $\frac{1}{2}$ w., IRC	T_2 = R.C.A. 6 F 6
R_6 = 100,000 Ω , 1 w., IRC	T_3 = R.C.A. 5 W 4

C_1 and C_2 = 0.03 μ fd, Mica Aerovox
 C_3 , C_4 and C_5 = 8 μ fd, Dry Electrolytic 450 v., Cornell Dubilier
 C_6 = 1 μ fd, Paper 450 v., Cornell Dubilier
 C_7 = 4 μ fd, Dry Electrolytic 450 v., Cornell Dubilier
 C_8 = 16 μ fd, Dry Electrolytic 450 v., Cornell Dubilier
 TR_1 = Inca C 61, 660 v., center tapped
 L_1 = Inca D 1, 45 ma. choke
Relay = 10,000 Ω , 0.015 w., Leach
 S_1 = two-gang, 6 pole non-shorting, Centralab

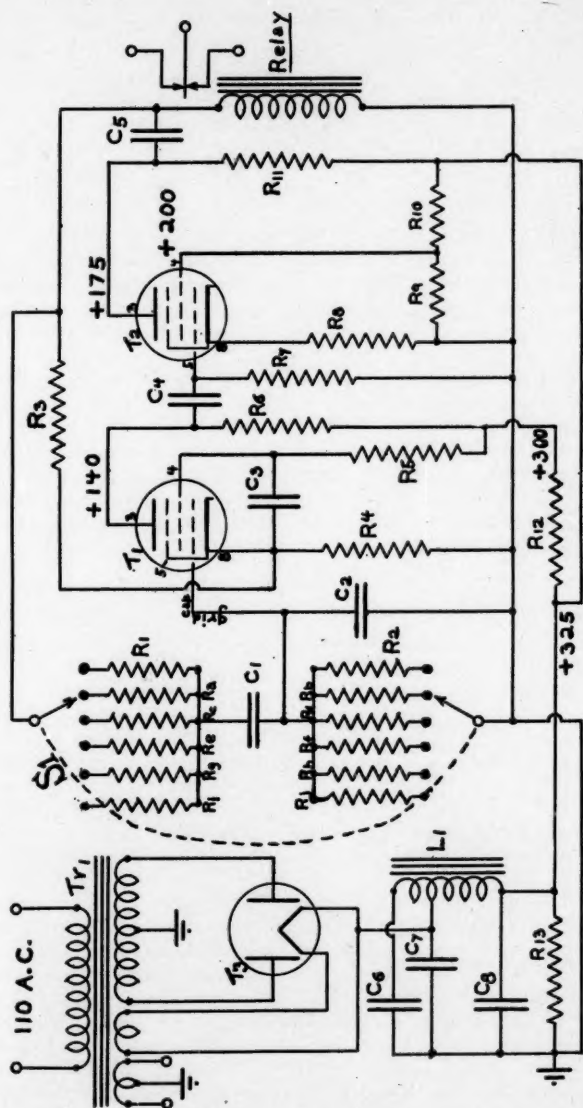


FIG. 1. WIRING-DIAGRAM OF OSCILLATOR
(For explanation of symbols see p. 600.)

which would necessitate some slight initial adjustment of the resistors to secure the exact times indicated.

Resistors	Oscillation rate
R_1-R_2	1 in 2 sec.
R_3-R_4	1 per sec.
R_5-R_6	2 per sec.
R_7-R_8	5 per sec.
R_9-R_{10}	10 per sec.
$R_{11}-R_{12}$	20 per sec.

Much slower rates of oscillation are not practical with this circuit since the capacity of the condensers and the magnitude of the resistors would have to be made disproportionately large. Faster rates are quite possible but most markers will not respond above 20 per sec., although there are a few on the market that will follow the 60~ current. We have arbitrarily adopted the above range since we feel that it is satisfactory for most purposes.

The circuit has been thoroughly checked and we find that the warming up effect, which is one of the difficulties with most of the slow frequency oscillating circuits does not exceed 1%. Neither line voltage fluctuations of as much as ± 10 v., nor a change in the number of cycles which would normally produce errors in a synchronous motor timer, will influence the accuracy of the timer. Apart from its accuracy and ease of assembly, one factor that should recommend the instrument, is its cost. The materials, not including the labor in assembly, will not exceed \$20.00 which is only a fraction of the cost of most timers in general use.

A NEW APPARATUS FOR THE STUDY OF BIRDS' LEARNING

By JOSEPH E. ZERGA, University of Southern California

Previous investigations of the learning-behavior of the bird in mazes have been restricted to patterns such as have been used with the rat, *e.g.* the horizontal multiple T-maze. Results of these investigations have shown the necessity of constructing a maze which will be suitable to the characteristic bodily structure and adaptive behavior of the organism concerned. Inasmuch as the bird is a perching animal, it is obvious that a reliable maze, if any maze may be said to be reliable, should contain perches.

The apparatus that is pictured and described here (see Fig. 1) was constructed especially for use with small birds; such as, the canary and sparrow. It is not only easy to construct but after it is made it may readily be changed from a maze to a multiple-choice box by removing the alley panels which are hooked onto the perches. The perches, which are 9 in. long and $\frac{3}{8}$ in. in diameter, are inserted into $\frac{3}{8}$ in. holes which are bored into the back of the box. Every perch is $3\frac{1}{2}$ in. distant from the next (there being 85 perches in number) and is easily removable from its socket thereby making it possible to create problem designs with the perches themselves.

The apparatus, excluding the goal and entrance cages, which are depicted with dotted lines, is 9 in. deep, 4 ft. high, and 4 ft. wide. The goal or entrance cages

are constructed with wire mesh and have a wooden bot'om. Each cage is hung before an entrance or exit that is cut in the sides of the apparatus. The front of the apparatus is covered with a wire netting, enabling the experimenter to follow every movement of the bird from the entrance to the goal cage. The easiest method of timing the 'runs' is to use a stop-watch, but the timing may be done automatically by attaching the goal perches to an electrical clock.

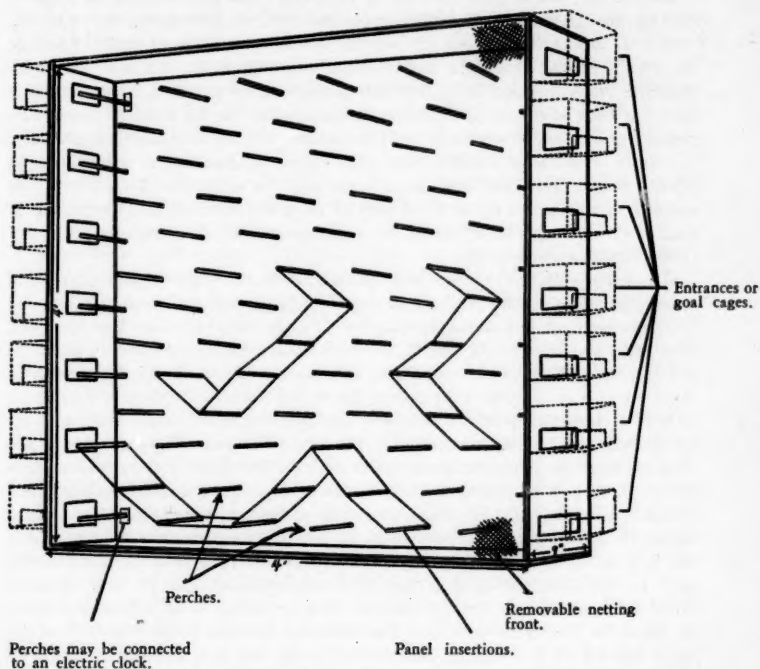


FIG. 1. FRONT VIEW OF THE APPARATUS FOR THE STUDY OF LEARNING IN BIRDS

NOTES AND DISCUSSIONS

PROBABILITY: A FUNCTION OF IDEOLOGY

Despite the vast amount of work in probability and predictability, the rôle of ideology, though an essential determinant in this field, has gone almost unrecognized. "The calculus of probability has received far more attention than its logic."¹ Ideology has come into the lime light with renewed emphasis in the last few years with regard to the experiments in extra-sensory perception, but its implications apparently have not been analyzed. Questionable generalizations in the field of chance and probability are applied wholesale to life situations with an astonishing complacency.

Chance of guessing correctly may, apart from considerations of information or psychic powers, vary, like other phenomena, with the individual. This theory does not seek a hearing merely as a criticism of an unfortunate statistical technique as used in the investigation of extra-sensory perception, but as a challenge to critical consideration in its own right.

To begin with, let us see how probability shifts with varying degrees of knowledge. Suppose that you read in the morning paper that a small ship, the *Sea Gull*, has been lost in a storm. Forty of the 50 souls aboard her have been drowned. You know that your friend, Mr. X, a retired business man, had booked passage on the *Sea Gull*. What is the probability that he was drowned? The probability is 40 in 50, or 4 in 5. What is the chance that he was saved?² The chance is 10 in 50, or 1 in 5. Information comes in slowly, but the next report states that 20 of the 40 persons drowned were members of the crew. The probability that Mr. X was drowned has now shifted to 20 in 30, or 2 in 3. The chance that he was saved is 10 in 30, or 1 in 3. Another bulletin comes in, reporting that 5 of the passengers drowned were women. The probabilities with reference to your friend have changed again. His chance for life is now 10 in 25 or 2 in 5, and the likelihood that he is lost is 15 in 25 or 3 in 5. At this point, information arrives from the ship's owners in a foreign land, stating that there were 20 passengers and 30 crew on board when the vessel sailed. Since 40 persons were drowned, 20 of whom were crew, all 20 of the passengers must have been drowned, including your friend. Then you learn that Mr. X had changed his mind, after all, and had remained at home. He is safe and well and at no time was there any possibility of his having been lost on the *Sea Gull*. Nevertheless, the *probabilities* shifted at different instants, even though there was only one *possibility* at any instant.

This is an example of the function of varying degrees of knowledge in determining the probabilities of the culmination of a given event. Probability may, then, be defined as the combination of knowledge and hypothesis between the limits of omniscience and nescience. There is no such thing as probability existing as an objective phenomenon in the universe. Possibilities exist objectively—independent of our thinking process, our degree of information, our ideology—but as soon as we introduce the concept of probability we refer to some state of knowledge or

¹ J. M. Keynes, *A Treatise on Probability*, 1929, 21.

² The words 'chance' and 'probability,' as used in this paper, are exchange terms denoting the likelihood of the culmination of a given event.

hypothesis. This being so, all probability is 'subjective' probability and may be different and valid for different persons with respect to the same event if based upon different knowledge or hypothesis.

To further illustrate the difference between *possibility* and *probability*, assume that a full deck of regular playing cards is thoroughly shuffled and placed face down on the table. No one has any knowledge of the order of the cards. What is the probability that the top card is the Ace of Spades? The probability is 1 in 52, since, so far as is known, any one of 52 cards *might* appear in the top position, of which only one could be the Ace of Spades. After the top card has been removed from the deck, but before anyone has looked at it, what is the probability that the *second* card is the Ace of Spades? The probability remains 1 in 52, since there are still 52 possibilities for that card *so far as is known*. This probability is constant throughout the deck. When the last card is reached the probability of drawing the Ace of Spades is still 1 in 52. There are still 52 possibilities for that card, *so far as is known*, although, as a matter of fact, drawing the Ace of Spades after a certain point may be *entirely impossible*. To anyone lacking the pertinent knowledge of possibility and impossibility, the *probability* remains constant.

If we *know* what is removed from the deck at each trial, and as a consequence, what remains in the deck at each trial, the probability shifts in accordance with this knowledge. If our knowledge remains constant, so does the probability, *for us*. If someone else has more or different knowledge, the probability *for him* will not be the same as the probability for us. This leads inevitably to the conclusion that probability is not the function of the empirical set-up, but of what is *known* or *hypothesized* about the empirical set-up.

In a preceding article,³ the present author has attempted to demonstrate the operational significance of probability in an empirical situation involving continual shifts in likelihood of occurrence with reference to a series of symbols to be guessed. It was assumed that the *probability* of occurrence for a given symbol varied concomitantly with changes in the *possibility* of occurrence for that particular symbol. Returning, for purposes of illustration, to the set-up previously employed, let us consider some additional aspects of the situation as a necessary preliminary to an understanding of probability as a function of ideology.

The deck of ESP cards⁴ consists of five symbols quintuplicated: Star, Circle, Cross, Oblong, and Wavy Lines. Imagine, once more, that the 25 cards are arranged before us in a pile, face down. Chance for a particular symbol to appear on the topmost card is obviously 5 in 25, or 1 in 5. However, each time a card is withdrawn from the deck there is a shift in probabilities *if we know which cards have been removed and which remain*. If we do *not* know which cards remain, probability of occurrence for any card remains a constant, since the possibility of occurrence for any card is also constant, *so far as is known*. This constancy of probability holds true for a person guessing the deck only so long as he responds like a machine, without the influence of memory, inference, or prejudice.

Now, let us suppose that, instead of asking some person to guess the deck, we employ an unbiased revolving disk. The disk is divided into five equal sections, each

³ E. A. Becknell, The function of dependent probability in ESP data, *J. Gen. Psychol.*, 19, 1938, 373-381

⁴ Extra-sensory perception cards employed by Professor J. B. Rhine and his colleagues at Duke University in their investigation of telepathy and clairvoyance.

of which is arbitrarily selected to represent one of the five above mentioned symbols. On each 'guess' the disk is spun at random and a 'match' between a given card in the deck and the symbol indicated when the disk stops spinning constitutes a hit. The entire deck of 25 cards is 'guessed' just as with the human individual. It cannot be denied that *possibility of occurrence* for any symbol in the pack still shifts as cards are removed, but will the *possibility of correct matching* on the part of the disk be a function of this shifting possibility of occurrence in the pack? It will not!

For instance, suppose that on the 20th trial there are no Stars left in the deck. Consequently, from that point on, it is impossible for a Star to appear in the deck. Suppose that the disk stops at a Star. What is the possibility of its matching the pack? Obviously, zero; but that is not the point. Rather, the point is what possibility the disk has of stopping at whatever symbol *does* appear at that position in the deck. We are interested in whether the disk *can* guess what is *in* the deck, not in whether it *will* guess what is *out* of the deck. No matter what is taken *out* of the deck, it is always equally possible for the disk to match what remains *in* the deck. This differentiation may seem hair splitting or irrelevant at first, but the questions are separate and distinct, and to confuse them is fatal to any real understanding of the problem. So long as the disk is guessing the cards and so long as one of the five cards represented on the disk remains in the pack, the disk has not only a constant *probability* at every point, but a constant *possibility* also. It cannot be otherwise.

However, in the case where one deck of 25 cards 'guesses' an identical deck of 25 cards, the *possibilities* do *not* remain constant. When one pack of cards guesses another pack, a card being removed from each pack after every trial, there may be *no possibility whatsoever* of matching after a particular symbol is used up in one deck but is still represented in the other. The *probability* is still constant so long as we do not know which cards are being removed. The *possibilities* of the two empirical situations may differ, yet the *probabilities* remain the same. This leads to the formulation of an hypothesis of probabilities at the same time quantitatively homogeneous and qualitatively heterogeneous. In other words, probabilities based upon different empirical set-ups might be numerically equal yet qualitatively unequal due to the presence of different *possibilities* in the empirical situations.

Just what significance differences in the possibilities of two set-ups may have in the face of the same probabilities, this writer is not at present prepared to state. It would be by no means a safe assumption that these differences in possibilities play no part in the end results in the presence of equal probabilities. A guessing situation in which the possibilities as well as the probabilities remain constant would seem to be more favorable, so far as correct responses are concerned, than one in which the possibilities shift unpredictably.

For this reason, any experiment in the guessing of one deck of cards by another, involving removal of cards from both decks, might, though it continued over thousands upon thousands of trials, prove decidedly dubious as a basis of comparison with the results of Rhine's students at Duke.⁵ Even disregarding, for the moment, factors of memory, rational inference, and prejudice in the human guesser, the two configurations are at variance. Consequently, use of data drawn from

⁵ J. B. Rhine, *Extra-Sensory Perception*, 1935; *New Frontiers of the Mind*, 1937.

thousands of matchings between such decks of cards as an indication of what the end results 'ought' to be for Rhine's subjects in a different set-up, might prove utterly meaningless.

On the other hand, suppose that a set of 5 cards, one of each symbol being represented, was matched against the deck of 25. One card from the set of 5 would be matched with one card from the deck of 25 at each trial. Whereas cards would be permanently withdrawn from the deck of 25, the set of 5 would be kept constant by replacing the withdrawn card after each trial, and shuffling all 5. In this case the essentials would be the same as for the disk situation and would approximate the set-up with the human guesser *if* prejudice, inference, memory, and other ideological factors were put aside.

Our problem would be far less bewildering if the human being responded exactly as the disk. In employing the disk we have, of course, a set-up of ideal simplicity. The disk, so far as we know, is not confronted with an ideology. Every card might just as well be present in the deck at each trial, so far as the disk is concerned. In other words, each 'guess' is made without bias and without regard for what may or may not be left in the deck at any particular point. The disk does not seek to remember its calls, employ rational inference, or outwit the pack in any manner. Each response is entirely independent of every other response. This is seldom true of the human subject. The person does not necessarily behave like a machine. Wherever we have a human individual we have also, presumably, an ideology. Hence, in a set-up such as Rhine's, probability becomes incomputable, since the subject may be responding to a series which *exists only in his imagination*, as well as to the actual empirical series.

This point has been touched upon in the preceding paper, but the following more important concept was not previously formulated: probability becomes incomputable, *not* by reason of the shift in possibility of occurrence, *but wholly by reason of the ideology*.

Here is how the situation stands: you will recall that in the case wherein the disk guesses the cards the possibility of occurrence with reference to the deck shifts as cards are removed. Possibility of correct matching on the part of the disk, however, remains constant. *Probability* of correct matching is also constant. In the case of the person guessing the cards the possibility of occurrence with reference to the deck shifts and the possibility of correct guessing remains constant, even as with the disk.

It is the conviction of the writer, however, that the probability of guessing correctly does not remain constant in the case of the person and that, since no explanation is available within the empirical set-up, it must be sought beyond—in the mind of the agent guessing. Since probability is a state of knowledge or hypothesis, and since knowledge and hypothesis are indisputably a part of our ideology, it can readily be seen that probability is a function of ideology. Differences in degree and kind of knowledge have been shown to change probabilities with reference to the culmination of a given event. But what about hypothesis? Just how can inference, prejudice, and memory of past hypotheses affect probability? When we hypothecate possibilities different from those which actually exist, and react in the light of these hypotheses as if they were reality, does this process affect our probabilities of correct prediction? Can two people with equal knowledge of the possibilities of the culmination of an event be said to have different proba-

bilities of correct prediction with reference to the event because of differing hypotheses?

Take the case of John and Bill, for example. Both boys set out to guess the deck of ESP cards, having been shown the five symbols and informed that each symbol occurred five times, making a total of 25 cards. The deck was shuffled and Bill began to guess. He made a mental note of each response as he called it and was careful to call exactly five of each symbol. When he had guessed five Crosses, for instance, he would not guess Cross again. He happened to call all five Crosses in the first 15 guesses, and since three Crosses occurred in the deck in the last 10 cards, there was no *chance* of his scoring a hit on those three trials, although the *possibility* always existed. In other words, he was responding both in the light of actual knowledge and in the light of an hypothetical series which he employed as if it were actual knowledge.

John, on the other hand, guessing the same deck with the cards in the same order, had a preference for Stars, and, deciding that a Star could appear at one point in the pack just as well as another, he called "Star" on 10 of the 25 trials. This meant that on 5 of the 10 calls he *had* to be wrong. The probabilities for the two boys differed because they combined the same knowledge with different hypotheses, resulting in differing ideologies. Obviously, the more nearly the empirical series and the ideological series coincide, the more hits will result.

To use an extreme illustration, suppose that the deck had been described to Bill before he started guessing, but had not been shown to him. If he imagined the set-up to be a hoax and suspected the cards of being blanks, calling "Blank" 25 times as a consequence, his hypothesis would rather materially affect both the probabilities and the end results.

In the long run, the effects of such hypotheses may cancel one another, but such cancellation over a large number of trials does not deny the existence of the effects before they are cancelled, and by no means vitiates the theory of their existence. In fact, instances of freak or 'sport' runs, either of 'hits' or 'misses,' in guessing situations, might well find explanation in this light.

As a final example of probability as a function of knowledge and hypothesis, consider the following set-up. Blanks for a True-False test are distributed among students in a college class. One hundred spaces are provided on each blank for the recording of answers, but no questions are presented. As a matter of fact, no questions exist. The subjects are instructed merely to guess, writing either "True" or "False" in each of the 100 spaces. Suppose that a key for checking responses is so constructed that 100 items are True and none is False. The subjects are acquainted with the fact that all the items run one way, but the direction of the bias is not revealed. Hence, providing they react in the light of the information supplied them, each student will make a score of zero or of 100. Next day the experiment is carried on again. Precisely the same key is used as was used the day before, but the subjects are otherwise informed. They are told that this time the distribution of True and False items is 50-50. If the subjects do not suspect this piece of misinformation and allow themselves to be guided by a false hypothesis, every student will make a score of exactly 50, neither more nor less. The empirical key is the same on both occasions. In the first case there is a possibility that the ideological series will coincide with the empirical series; in the second case there is none.

Assume that the distribution is 80-20 and that the subjects are told that it is 80-20. Then their scores may fall anywhere between zero and 100. On the other hand, if they are told that the distribution is 50-50 and are again confronted with a false ideology, the range of scores will be 30-70. They will not make less than 30 nor more than 70. Such examples can be continued *ad infinitum* and can be worked out for any desired combination of distributions.

In conclusion, let us look for a moment at the questions of subjective and objective probability and the nature of uncertainty. Keynes⁶ quotes the following statement of the philosopher, Hume: "Probability is of two kinds, either when the object is in itself uncertain, and to be determined by chance; or when, though the object be already certain, yet 'tis uncertain to our judgment."⁷ Upon reading this declaration the question uppermost in the mind of your author was: just what sort of uncertainty can there be except that which is uncertain to our judgment? How can an object be "in itself uncertain?" What meaning can the term "uncertainty" have except with reference to our judgment? If we state that it is uncertain whether or not it will rain, we mean that it is uncertain *to us*, or to the weather man, or to someone else. Possibilities exist apart from us, but uncertainties and probabilities do not. Just as there would be no sound if there were no organ capable of hearing it, so there can be no uncertainty without mind capable of *being uncertain*. Uncertainty is not an objective phenomenon; uncertainty is a subjective experience. It is not the event which is uncertain; it is *we* who are uncertain *about* the event. If we knew all the facts there would be no uncertainty.

Perhaps that probability in which only knowledge is a factor might be considered more objective than that probability in which hypothesis is likewise a factor. Knowledge, with reference to many situations involving human beings, is a factor capable of control, but the hypotheses with reference to the same situations may be entirely beyond control. With a machine doing the guessing, however, we can eliminate the hypothesis angle from the field of the response. Hence, the machine situation might be considered an example of a more objective probability than one involving a human responder. This conception of two kinds of probability might partially satisfy Hume, although perhaps his differentiation may be better expressed in the phrases "probability before the fact" and "probability after the fact."

We have rambled a long way in this discussion, finding that truths are still elusive and significances nebulous. Just how much do we really know, after all, concerning what we are pleased to term 'chance' and 'probability'? Have we been so intent upon the ultimate smoothing out of distributions of 'chance' events that we have failed to understand the nature and significance of the single events composing the distributions? Have apparent simplicities blinded us to the real complexities? Without ideology probability is meaningless; it cannot exist. Likelihood of the culmination of a given event is only another name for what we know and what we think about the culmination of that event.

It becomes increasingly evident that before we venture to make comprehensive generalizations in this field, we should look a little more closely to the untraveled labyrinths of ideology.

Florida State College for Women

ELIZABETH ANN BECKNELL

⁶ Keynes, *op. cit.*, 282.

⁷ David Hume, *A Treatise on Human Nature*, Book ii, part iii, section ix.

THE STABILITY OF THE *IQ*

It is evident that science does not go far without theory. There are times when it is truly remarkable how far it does go; at such times, however, the theory is present but unexpressed. That is to say, we act and design experiments as if we explicitly believed in a given theory. The current discussion about the constancy of the *IQ* is a case in point. The facts would gain a great deal in clarity with the aid of a little explicit theorizing. We have here a set of well-authenticated facts which prove that the *IQ* is not influenced by the environment. There is also another set of well-authenticated facts which demonstrate that the *IQ* is influenced by the environment. Everyone knows that 'facts' never lie; they do, however, have the distressing habit of contradicting each other unless they are embedded in a conceptual matrix.

Instead of attempting to construct the necessary matrix it happens only too often that we stubbornly deny the existence of the facts we do not wish to believe. At all events, we try to belittle them; the critical ratio is too small, or there are uncontrolled variables.

As regards the *IQ* it is difficult to understand what the 'hereditarians' have in mind when they contend that existing data does not clearly demonstrate a co-variation of environment and *IQ*. No data are needed other than the fact that if a person is coached on the test he is to take five minutes before the appointed time, his *IQ* is certain to be increased. It is to no purpose to say the test material does not fulfill its functions if the testee has had *direct* experience with it. For the test material also does not fulfill its function if the testee has had *no indirect* experience with it; if, for instance, he does not speak English, if he doesn't understand the word which is used, if he has never seen or heard of the object or event which is in question. Thus, if we ask a child a question involving apples, and he has never heard of apples, it is contended the question is an unfair one as a test of his native capacity; but then if we turn right around and ask him the same question shortly after giving him the correct answer, we still insist that it is an unfair test of his capacity! Surely there is something amiss here. How much experience must a person have with the object involved in a question, before the question tests his native ability? However we attempt to resolve this difficulty it is obvious that we are forced to conclude a person's answer to a question is always influenced by his environment.

It would seem that the whole difficulty arises from the failure to appreciate the fundamental fact that ultimately a psychological test measures *performance* and can never measure anything else. This fact, taken in conjunction with another equally fundamental fact, namely, that hardly any human performances are instinctive, leads inevitably to the conclusion that practically all performances must be influenced by the individual's past history, and hence that the scores in all psychological tests are inevitably influenced by the individual's past history.

The impulse at this point is to make the accusation of extreme 'environmentalism.' There can be no doubt, of course, of the ubiquitous influence of the environment upon performance; but to go from this to the proposition that an intelligence test score reflects only a person's environment is analogous to saying that the area of a rectangle is due to its length. To put the matter formally, the preceding considerations merely demonstrate that environment is a *necessary* factor determining the *IQ*; it is not yet demonstrated that the environment is also the *sufficient* factor.

The question, then, is not whether the *IQ* is or is not influenced by the environment. Rather, we must ask, "When may the *IQ* be taken as measure of the 'native ability' of the individual?" It would seem that many of the discussions on the *IQ* constancy beg the question; it is frequently assumed that the *IQ* measures 'native capacity' willy-nilly. If we are to answer the above question, however, it is evident that we must ask and give some sort of answer to a more fundamental question; namely, "What are the variables which determine the *IQ*?"

Fortunately, the question is not as formidable as it sounds. We approach it by assuming the most general case; namely, that an individual's mental age (*MA*), *i.e.* his performance, is dependent both upon his native ability (or heredity) and his past history (or environment). Utilizing the familiar symbols *H* and *E* for these two classes of variables, we write accordingly

$$MA = f_0 (H, E) \dots\dots\dots [1]$$

where the factor of chronological age (*CA*) is kept constant for all the subjects described by the equation. We do not need to know the exact nature of the relationship between these three variables; for the task which faces us here, the equation as it stands is sufficient.

The problem now resolves itself into the answering of the specific question: "Is equation [1] correct?" This may be determined by testing the two following deductions: (1) The *MA* of two persons having the same *CA* and the same *E* may not be identical; (2) the *MA* of two persons having the same *CA* and identical *H*'s may not be identical. If both these deductions are correct, the assumption of equation [1] is justified, in which event the whole *IQ* constancy controversy is immediately shown to be trivial and out of focus, since it becomes analogous to a debate as to whether a force is uniquely determined by the acceleration imparted to an object.

Presumably the second deduction has been corroborated to everyone's satisfaction by the well-known instances of identical twins raised apart. Our problem, then, degenerates into the still more specific one of determining the correctness of the first deduction.

The testing of this proposition, however, is a peculiarly difficult task, and its difficulty has been exploited to its fullest extent by the extreme environmentalist who feels free to contend that it is never possible to state that two past histories are identical. This makes it possible for him to hold that ever discrepancy in *MA* (with *CA* fixed) reflects a discrepancy in environment. Even the 'constancy' of the *IQ* cannot be used as an argument against him, since all he needs to do is to point out that the constancy of the *IQ* in a given case merely reflects the 'constancy' of the environment. Hence if there be two persons whose *IQ*'s are *x* and *y*, respectively, and these remain *x* and *y* even for long periods of time, this constant discrepancy reflects a constant discrepancy in the environments of the two individuals.

All the critical ratios, correlation coefficients, and *t* values in the world are helpless against a fundamental logical obstacle of this sort, since they are completely irrelevant. It is plain, however, that the 'insuperability' of the environmentalist's position is its greatest weakness. As it stands, it becomes dangerously near to that class of hypothesis which asserts that if one had sufficient faith he could defy the force of gravity. The complete uselessness of the hypothesis lies, of course, in the

undefined word *sufficient*, since its lack of delimitation always permits one to explain a given failure by asserting that the faith was not sufficient in that case.

In order, therefore, to break the vicious circle it is necessary to define the phrase *equality or equivalence of environment*. Once a definition of this is given, Deduction (1) becomes verifiable, since an unambiguous answer is available through the ordinary channels of experimentation.

With this end in view, the following definition of the equivalence of two environments is proposed: Two environments E_1 and E_2 are equivalent with respect to a given test when a pair of identical twins raised in E_1 and E_2 , respectively, give the same *MA* on the test in question. We may, indeed, formulate a more generalized definition of what might be called *Environmental Distance* of which the above definition is a special case.

The distance between two environments E_1 and E_2 is equal to the difference in the *MA*s of a pair of identical twins raised in E_1 and E_2 , respectively, when the lesser of the two *MA*s is equal to j (j being an arbitrary value). It will be noted that while the second of the two definitions appeals by virtue of its greater generality and usefulness, it becomes necessary to 'fix' the *CA* and one of the *MA*s in order to make the definition unambiguous.

It now becomes possible to make definite experimental tests of Deduction (1); namely, that the *MA*s of two persons having equivalent E s and the same *CA* may not be identical. The only consideration remaining is that of satisfying oneself that the subjects of the experiment fulfill the specification of the equivalence of E . If this is done, then we merely need a group of children of identical ages which are brought up in equivalent E s. The above proposition is corroborated if the standard deviation of the *MA*s of the group is greater than zero. In this event, it is demonstrated that the *MA* reflects native ability under conditions of equivalent environment and equation [1] is justified. If, on the other hand, the standard deviation is not significantly greater than zero, we must conclude that *MA* reflects environment alone and that equation [1] is not true. We should then be forced to write

$$MA = f_1(E) \dots\dots\dots [2]$$

and the environmentalists would be complete masters of the field.

We are now in a position to consider the matter of the 'constancy' of the *IQ* directly. From actual studies we know that the *IQ* is not too constant, since correlations between *IQ*s which have been obtained at intervals of more than three or four years are seldom over $+0.80$. It is customary to attribute this to 'errors of measurement' and to 'uncontrolled variables,' but this is not desirable until we have done everything possible to straighten out the matter conceptually.

In discussing the question as to whether the *MA* could be considered as reflecting H or E or both, it was possible to keep *CA* constant, and hence the function for *MA* took on the simple form given in equation [1]. In considering the question of the *IQ* constancy, however, this is obviously not possible, since we have to deal here with what happens to the *MA* of the individual with an increase in age. In this instance we must take into account the factor of maturation, and hence we must alter equation [1] accordingly. We might, therefore, be tempted to write

$$MA = f_2(H, E, M) \dots\dots\dots [3]$$

where M represents the maturation factor.

It is, however, desirable to examine variables rather sharply, since equations,

even when expressed quite formally, as in this case, require something more than the mere writing down of letters within parentheses. It is clear that the factor of maturation, stripped of all its barnacles, implies nothing more than a change of something with a change in time. Hence we should substitute the dimension T for the dimension M. By so doing, we say in effect, that when H and E are held constant, a change in the value of T is accompanied by a change in the value of MA. If maturation is to mean anything as a separate variable, it ought to mean this. We shall write, therefore,

$$MA = f_3 (H, E, CA) \dots\dots\dots [4]$$

if it is assumed that heredity as well as environment is a factor in the determination of MA. Utilizing all of the considerations which have preceded, we may now define the IQ in the following manner,

$$IQ \equiv MA/CA = f_3 (H, E, CA)/CA \dots [5]$$

We are in a position now to ask a very instructive question: "How is it possible for the IQ to stay constant at all?" We may answer this question quite simply if we are willing to make an assumption; namely, that the H for a given individual does not change during the course of his lifetime. This may seem a rather drastic proposition, but it is a common-place fixture of genetic theory. If H is assumed to be constant, it is evident from equation [5] that the IQ for any given individual remains constant only when E changes in time by an amount such that the ratio of the numerator to the denominator maintains a constant value.

This may be expressed in different language by saying that equation [5] implies what might be called an 'Environment Schedule' for the individual if the condition of an IQ constancy is to be maintained. We thus see that the IQ, far from being uninfluenced by the environment, requires, on the contrary, that the environment change at a definite rate if it is to stay constant at all.¹ The corroboration of such a view is to be found in such work as that of Gordon with Canal Boat children. The corollary of the above is also evident, namely, that if the 'Environment Schedule' is exceeded, the IQ will fluctuate upward; if it falls short, it will fluctuate downward.

In light of these considerations, it is not surprising that the correlation coefficients between IQs obtained at different intervals are not higher than those reported. On the other hand, the very process of evaluating the items in a Mental Age Scale and the procedure of standardization itself guarantees a rough constancy of IQ unless the individual's environment suffers a marked dislocation. Consequently, if an individual's 'Environmental Schedule' does not fall outside the limits of those characterizing the persons forming the group upon which the test was originally standardized, we can expect that there will be no dramatic alteration in his IQ. As in the case of the MA, however, it is clear that the identity of IQ in two persons can be taken as an indication of equivalence in their native ability (or H) only in so far as their environments may be said to be 'equivalent.'

It would seem to be indicated by the foregoing, that the current debate in regard to the stability of the IQ suffers from a lack of sufficient conceptual dissection. Even a superficial analysis disposes one to feel that statements like "The IQ reflects an individual's native ability" or "The IQ reflects an individual's environment" are

¹ In the event $f_3 (H, E, CA) \equiv (H) (E) (CA)$, then the IQ remains constant only when the environment does not change its value.

in the nature of "incomplete sentences" and come very close to repeating the old squabble as to whether it is "Heredity or environment that determines human nature."

University of California at Los Angeles

J. A. GINGERELLI

A CAUTION ON THE USE OF THE METHOD OF CORRECT MATCHINGS

The mathematics of the procedure by which two sequences of items are paired for correct matchings has been obtained and quite adequately presented by Chapman.¹ In order to inform the reader of the general nature and use of the correct matching method the following paragraph is quoted from Chapman:

A method common to many experimental and testing procedures in psychology and education is to require an individual to match, as best he can, members of one series of items with members of a second series of quite different items certain of which are in some sense true opposites of items in the first series. Thus the experimental psychology of personality has often investigated the ability of graphologists or laymen to pair samples of handwriting produced by a group of persons with, say, character-sketches of these same persons; and the excess of correct matchings thus produced over the number to be expected by chance has been used as evidence that the expressive movement of handwriting affords characteristics diagnostic of personal traits. Fortunately, the excesses experimentally obtained have often been so large as obviously to exclude the operation of chance alone. But many empirical results show small excesses only; and the interpretation of such findings has not hitherto been subjected to rigid statistical analysis.²

In their book on *Studies in Expressive Movement* Allport and Vernon remark that "it is not easy to eliminate all the spurious elements which make for correct matchings."³ But Chapman does not point out the possibility of these spurious elements in the application of his theory noted by Allport and Vernon and to which his examples and those of Powers are subject.⁴ The particular use of the method to which I would give caution is that of Powers and Chapman in using multiple judgments on the same data as a means of obtaining an indication of some functional relationship other than chance. The use of many judges is evidently intended to give statistical significance through numbers of cases in a matching situation where the effects of chance cancel out sufficiently while retaining the possibly slight true functional effect. But is this necessarily or even probably achieved? Let us note briefly the mathematical situation.

The mathematics which Chapman derives pertains to two identical packs of distinct cards randomly matched. Let such decks be thus matched N times. He obtains the moments of the mean number S of correct matchings per time. Of these the mean is 1, and the standard deviation of S is $1/N^{1/2}$ which can be made as small as desired by choosing a sufficiently large N (the number of judges in the applications). Then utilizing a Type III approximation he obtains the probability that the average would have been as large as S .

Now suppose that N judges take the same two sequences of items and attempt

¹ D. W. Chapman, The generalized problem of correct matchings, *Ann. Math. Stat.*, 6, 1935, 85; The statistics of the method of correct matchings, this JOURNAL, 46, 1934, 287ff.; The significance of matchings with unequal series, *ibid.*, 48, 1936, 167ff.

² Chapman, *op. cit.*, *Ann. Math. Stat.*, 85.

³ G. W. Allport and P. E. Vernon, *Studies in Expressive Movement*, 1933, 202.

⁴ *Op. cit.*, 212.

to pair similarities. To consider an extreme case suppose there is one accidental similarity occurring at a correct or matched pair, there being no other such similarities. Then, the judges observing it as they are supposed to, the expected average is now 2 instead of 1. The observed mean will then obviously be significantly different from 1 with even a small N . The small probability obtained in this case, while a reliable measure of the reactions of the judges collectively to properly paired stimuli, cannot be said to be indicative necessarily of anything but the one chance similarity, which might itself have a high probability of occurrence.

In two arbitrary sequences of items to be paired (correct pairings pre-determined) there usually exist pairs (either matching or non-matching) which several of the judges would likely adjudge as similar, in particular if the basis for judging similarity is a rational one. Thus a great variety of possibilities exists in which it is not correct to assume that the chance mean is 1. It might be greater and it might be less than 1.

A use of the method to diminish the effect of the random similarities is to increase the number of sets of data and to allow effectively (as far as the evaluation is concerned) only one judgment to each set of data. The standard deviation of each set is then 1 instead of $1/N^{1/2}$. The several sets of data could then be evaluated properly using Chapman's method wherein N equals the number of sets of data instead of the number of judges. The resultant probability would then be a measure of the aforementioned functional connection.

The use of many judges to each set of data would furnish a reliable measure of the number of correct judgment matchings which the two sequences of items to be paired exhibit, be they chance or caused by a true functional relationship. This estimation which is an average number of correct judgments (the estimation is performed on each set of data) will usually be non-integral, but since a Type III curve is used in the final evaluation, interpolating S (which is then the average of all the above averages) would appear to be quite in order. The necessary use of 1 as a standard deviation for each set of data regardless of the number of judgments upon it is a crude though safe value but may nullify the utility of the method.

University of North Carolina

JOSEPH A. GREENWOOD

THE FIFTEENTH ANNUAL MEETING OF THE MIDWESTERN PSYCHOLOGICAL ASSOCIATION

The Midwestern Psychological Association held its fifteenth annual meeting at the University of Chicago, Friday and Saturday, May 3 and 4, 1940. The official register was signed by 565 members and visitors.

At the annual business meeting on Friday afternoon, 82 new members were elected, bringing the present membership list to 517. The election of officers was announced with E. K. Culler, University of Rochester, President for the year 1940-1941; members of the Council, H. F. Harlow, University of Wisconsin, 1940-1943, and A. W. Brown, Institute for Juvenile Research, Chicago, 1940-1941, to fill out the unexpired term of E. K. Culler. It was announced that the next annual meeting of the Association will be held at Ohio University late in April or the first week of May, the exact date to be announced later.

Eleven sessions were held at which 82 scheduled papers were read. The sessions included those on general psychology, electrophysiology, animal learning, mental

tests, attitudes, sensory and motor phenomena, clinical psychology, psychometrics, human learning, genetic and comparative psychology, and personality. In addition, nine symposia were conducted on conflict and adjustment, analysis of variance, current behavior theory, professional problems in psychology, contemporary social problems, the first course in psychology, motivation and tension, mental hygiene, and a summary of laboratory reports.

At the annual banquet held on Saturday evening, E. S. Conklin presided. Vice President Emery T. Filbey of the University of Chicago, gave the address of welcome. J. P. Guilford gave his presidential address on "Human Abilities," a critical review of recent experimental and statistical findings.

A resolution passed at the business meeting requested the Secretary to thank the University of Chicago, the host department, and members of the local committee headed by Professor Forrest A. Kingsbury for the excellent facilities and entertainment provided in these meetings, which set a new record for attendance and new members.

Northwestern University

ROBERT H. SEASHORE

THE LOS ANGELES MEETING OF THE WESTERN PSYCHOLOGICAL ASSOCIATION

The 1940 meetings of the Western Psychological Association were held June 14 and 15 in the new Psychology Building at the University of California at Los Angeles. The new building, with its 73 rooms devoted to psychology, was to be formally celebrated on June 13 by appropriate ceremonies. On this occasion R. B. Loucks talked on physiological psychology, E. R. Hilgard on experimental psychology, Milton Metfessel on criminal psychology, and G. M. Stratton on the social psychology of war.

Thirty-seven papers were presented during the meetings in addition to the address of retiring President Howard R. Taylor, who spoke on "Dimensions of Scholastic Aptitude," and a symposium on the topic "Should Departments of Psychology Have a Service Function in the College Personnel and Mental Hygiene Program?"

At the business meeting, a resolution was tabled which requested that the Association seek to restrict in California the use of the term "psychologist" in the classified directory of telephone books to persons certified by heads of departments of psychology in universities. A resolution was passed calling for the support of the "American Standard" Work and Assistance Act, H.R. 8615, having to do with relief appropriations for professional workers. A motion was made by Knight Dunlap that a committee be appointed to investigate what functions psychologists should fulfill in national military organizations in times of peace and war in the interests of national defense. After considerable discussion, it passed with the amendment that the task had also to do with the preservation of peace, and with the understanding that this act did not assume that our participation in the present war is inevitable. The motion also authorized the committee to advise, consult, and cooperate with the American Psychological Association, the National Research Council, and other organizations as may be necessary for the development and effectuation of plans. The committee includes: K. Dunlap, E. R. Hilgard, and M. Metfessel, Chairman.

Officers elected for the coming year were: President, Mary B. Eyre, Scripps College; Vice-president, Roger B. Loucks, University of Washington. The Association is scheduled to meet at Berkeley, next year, at the University of California.

University of Washington

RALPH H. GUNDLACH

William Frederick Book: 1873-1940

William Frederick Book died May 22, 1940, at his winter home in Long Beach, California. He was born in southwestern Indiana near Princeton, on June 10, 1873. His experience as a teacher in the public schools prior to his study at Indiana University, from which he graduated in 1900, influenced him to become a student of educational psychology, the field of work in which his contributions are well known.

After studying at the University of Chicago, he entered Clark University in 1903, and was awarded the Ph. D. degree by that institution in 1906. In that same year he was appointed Head of the Philosophy Department at the University of Montana and established soon thereafter the psychological laboratory in that institution. He left there in 1912 to return to Indiana University as Professor of Educational Psychology. In the period 1913 to 1916, while on special leave of absence, he organized under the State Board of Education the department of Vocational Education in Indiana. From 1917 until being forced to retire in 1934 because of poor health, he served as Head of the Department of Psychology at Indiana University. He was co-editor of the *Journal of Applied Psychology* from 1920 through 1927.

His best known contributions were in the field of human learning, particularly the study of the development of skills represented by his now classical studies in learning to typewrite. The results of his early work in the analysis of the learning curves which were derived from these studies have had broad application to educational psychology in general.

Although intensely interested in the theoretical aspects of the psychology of learning, his activities were directed since early in his professional career towards the very practical matter of developing skills in the individual. He was keenly appreciative of the significance of basic individual differences as they are usually considered, but at the same time he was emphatic in the point of view that, for the great majority of persons, the differences in levels of efficiency and adjustment are to be accounted for primarily in terms of their skills. This point of view he applied not only to performance in the basic school subjects but also to such broad areas as those which he termed personal efficiency in the day's work—areas to which, as his publications clearly indicate, he gave considerable attention during the later years of his professional activity.

Under his direction the laboratory at Indiana University made definite contributions to the fields of mental testing and clinical psychology. Over a period of years he was an influential member of several important committees concerned with educational and social programs. Although his goal was never quite achieved, he worked assiduously for the establishment of a Child Welfare Research Station at the University. The establishment of the Psychological Clinic in the James Whitcomb Riley Hospital for Children, Indianapolis, in 1925, however, was a prized achievement of his administration.

Indiana University

EDGAR L. YEAGER

BOOK REVIEWS

Edited by JOHN G. JENKINS, University of Maryland

General Psychology. By L. E. COLE. New York, McGraw-Hill, 1939. Pp. 12-688.

If a psychologist writes a textbook nowadays, he should address it to some well defined class of students. This book seems to be intended for juniors, who have at least a sound cultural acquaintance with the natural sciences and with the elementary mathematics which they presuppose. The author obviously strives to employ the laws of physical causation wherever he can. I personally question whether the facts which interest most psychologists are best described in accordance with these causal laws; but if anyone feels bound to try to use them, to the neglect of laws of mnemonic telic causation, this book is written for him.

Cole's background is physiological; and most of his book is written from the viewpoint of an observer of other persons. He does not, however, choose a single set of postulates, such as those of a consistent behaviorism, and try to describe everything according to the rules which they determine. In this, I think he has done wisely, although one might wish that he had specified his rules of description and mentioned some alternatives.

Some 144 pages, comprising about one-fifth of the book, are devoted to certain problems in nerve-physiology in relation to behavior and conscious experience, and to a description of the structure and functions of receptors, conductors, and effectors. Pre-medical students, fresh from courses in comparative anatomy and physiology, probably will like this treatment.

Eventually, we may all ask what light physiological procedures and results can throw on human actions and conduct, except on a few simple reflexes. It is, of course, mentally stimulating to think of a person's behavior and mental content as expressions of some hypothetical temporal pattern of receptor-conductor-effector activity; but unfortunately there are no feasible operations that enable us to test the hypotheses. One, therefore, wonders whether the hypotheses, after all, are fertile. Meanwhile, we should ask whether the author has presented a set of accepted facts of observation consistent with these rules. Usually, at least, he seems to have done so.

The first chapter purports to be a history of brain physiology in relation to "doctrines of animism." The material seems to be derived principally from sources of the second order and higher. The chapter does not ornament the rest of the book. In it, and thereafter, Cole often asks where the mind, consciousness, thinking, emotion, personality, etc., "reside." The true answer, as he occasionally suggests, is "nowhere." I wish that he had pointed out, once and for all, that *relationships* (such as these) do not *exist*, anywhere or at any time; that they simply *are*, and that they are merely exemplified in those particular instances in which they appear as connectors of particular terms. When one talks, as some neurologists do, of functions "residing" in some bodily locus, one is reifying and personifying functions. For example, one neurologist recently suggested that *because* certain animals can be retrained in certain acts of skill more readily than when they were originally trained, provided certain regions of the cortex remained intact but not if the same

regions were surgically destroyed; *therefore*, the "seat of retention" of these acts lay in these specific regions. One critic pointed out that this is only another way of asserting that an if-then relation connects the integrity of the brain structure to the persistence of retention. He compared it to an incident in the apocryphal history of the boyhood of Sir Walter Scott. Young Walter, who was then a prig, desired to stand at the head of his class, but another boy always surpassed him in rote-recitation. But young Walter, noticing that this boy always fingered a certain button on his waistcoat while he recited, seized the opportunity to cut off the button one morning while the boy was saying his prayers. When the boy next stood up to recite, he felt for the button and missing it, failed ignominiously in his recitation, allowing young Walter to move up to the head of the class. "Now," asked the critic, "according to the neurologist's rule of description, must one not say that the boy's memory 'resided' in the button? When he has it, he remembers again; when he loses it, he fails; when he recovers it, he remembers again. Since his memory comes and goes with the button as the rat's memory remained or vanished with the cortical patch, are not the two instances perfectly analogous?"

Cole presents sensation, not as a separate topic, but incidentally in the chapter on receptors and their functions. This chapter is well done except for a few outstanding inaccuracies. For example: the mechanism of accommodation is described in Helmholtzian terms and not according to modern fashion; the millimicron is mistakenly denoted by the symbol $\mu\mu$ instead of $m\mu$; it is asserted (p. 126) that if two complementary lights are mixed in suitable proportions, their characteristics of hue are mutually cancelled, while the total brightness of the mixture lies between the brightnesses of the components. The fact is that the two brightnesses summate according to the laws of arithmetic addition, except for distortions which characterize Purkinje's effect.

The chapter on development classifies the nature-nurture and heredity-environment problems as spurious. The one on motivation includes a discussion of bio-energetics; the telic characteristics of motivation; the rôle of socialization; attention, including postural sets and preparation; Adlerian compensation; and volitional resolution of conflicting tendencies. For a 'free' will, Cole has no use; but he does not tell us what 'freedom' is.

The chapter on learning is 70 pages in length. Cole would like to explain learning in terms of physical changes in the organism resulting from prior stimulation and responses, but he admits (pp. 373, 440) that this cannot now be done. He exhibits and condemns the trial-and-error hypothesis, a Gestalt hypothesis, and a conditioned reflex hypothesis finding them all unsatisfactory. He does not consider the hypothesis that one skill can be learned by practicing others.

In the chapter on thinking, which includes memory, hypnosis, rationalization, etc., Cole devotes 25 pages to Freud's proclamations. I think he appraises them justly; in other words, he has no use for them. Again in this chapter, he asks where is the locus of thought.

His chapter on emotion is chiefly a description of the autonomic nervous system, smooth muscles and glands and the functions which physiologists now tentatively assign to them. He does not care for James, Lange, or their theories.

Fig. 76 (p. 458) presents a map of the teleo-stereoscope showing a path which light has never been known to take. Like many other authors, he borrows this diagram from Titchener without examining it.

The chapter on intelligence indicates many important discernments; but it does not mention the very strong evidence against constancy of the *IQ*. The chapter on personality is disappointing in that it assembles the results of many sham experiments uncritically. Examples are found in the treatment of Jung's, Kretschmer's, Heidebreder's, and D. A. Laird's 'types.'

For the class of students which I mentioned at the beginning, the book is likely to be useful. The style is for the most part clear and unembellished. The format is excellent; the index fair.

Tulane University

H. M. JOHNSON

L'hygiène mentale et l'éducation. By L. M. RÉGIS, A. BARBEAU, E. C. WEBSTER, NOËL MAILLOUX, A. G. BILLS, A. MARCOTTE and J. A. LONG. Ottawa, Canada, Les Éditions Du Lévrier-College Philosophique et Théologique Des Dominicains, 1940. Pp. 187.

The first annual report of the new association initiated by members of the Dominican order for the study of scientific methods in education, represents a happy collaboration, between Canadian and American psychologists, as well as a sane and balanced combination of the philosophical and scientific points of view. The object of the association seems to be primarily the study of educational problems in their French Canadian setting, so that the articles of Régis, Barbeau, Mailloux, and Marcotte are in French, and have a local reference as well as general value. The other papers are in English. All of the papers, but from very various angles, are concerned with the problems of mental hygiene and education.

The introductory essay by Father Louis-Marie Régis treats of the exact significance of this new Canadian initiative in scientific education. It is naturally an essay in orientation, discussing in a broad philosophical way "the genetic elements in the concept of education, the relation between education and the science of the individual, and the value of scientific methods completing the science of the individual." Father Régis sketches accurately the proper rôle and limits of metaphysics, the philosophy of human nature, and experimental psychology, in the study of the individual, and finds ample justification for all these approaches. Within the broad framework of the program suggested by Father Régis, a number of papers bearing on the relations of mental hygiene and education appear at the three following conferences.

Dr. Barbeau's study of "The Child and Criminology" includes discussions of the child at the bar of justice, as witness, victim, or delinquent, outlines the principles, governing the institution and conduct of children's courts, and offers statistics of child delinquency at Montreal. He studies the psychological, biological and sociological courses of delinquency, dismisses all concepts of biological fatalism, and relates the problems of prevention and cure of delinquency to the more general problem of an education at once in sympathy with the child and scientific in its methods.

Professor Webster of the Psychological Institute of Montreal arrives at the same conclusions through an analysis of the concepts of personality and character. He briefly discusses the trends of research and personality, criticizes the impractical character of most definitions of the term, and urges the acceptance of a workable definition in terms of actual behavior and concrete goals—a procedure illustrated in recent studies by Doll and Link.

The reports of the third session of the Congress are of particular value and broad

theoretical import, appealing as they do to all psychologists and psychiatrists, whatever their special field of interest.

Father Noël Mailloux presents a concise yet comprehensive study of the concept of sexuality. He offers a subtle philological and logical analysis of the term and its component elements in their genetic setting. While acknowledging the valuable services of psycho-analysts as observers of the sex life of man, he does not subscribe to their fundamental doctrine, and corrects certain concepts, such as that of the polymorphic-perverse character of infantile sex. For this he would substitute the terms polymorphic pervertible, suggesting mere possibility under certain circumstances, rather than an inherent nature governing the process of integration. Mailloux, believes that the problems of infantile sexuality may be solved by a tolerant and sympathetic application of modern methods for studying the individual. The child here as elsewhere must be guided always in the light of his actual capacity to understand, through the concrete material of socially and morally desirable habits, to the sense of values and ideals, and the final possession of responsibility, moral competence, and social usefulness.

In another paper of great theoretical value, Dr. A. G. Bills, discusses the principle underlying the hygiene of mental work. This is a remarkably concise and luminous account and interpretation of recent studies of work and fatigue, many of them by the author, or by his pupils. A number of unpublished researches are included. The author classifies methods of studying fatigue, points out their various limitations and the conflicts and agreements in data yielded by the various methods. It is a field that presents basic facts for mental hygiene, and, brings out clearly the point generally agreed upon by members of this congress, *viz.*, "that the mind is so intimately associated with the entire organism in its operation that it is impossible, to consider the one independently of the other" (p. 146).

The papers of the fourth and last session deal respectively with the "Practice of Mental Hygiene in the School," and "The Rôle of the Teacher in Popular Education." These papers are of a more popular character but present, nevertheless, points of view and criticism of much value. Under the first heading, Dr. Marcotte offers a classification and analysis of abnormal types of children found in the school, *viz.*, those handicapped by bad physique, or low intelligence, and discusses the behavior problems arising from the presence of these two types. A brief discussion of the neurotic and the socially mal-adjusted is followed by the sketch of a possible program for mental hygiene in the schools. In conclusion Dr. Long offers some considerations on the formation and education of character based on recent experimental studies and the view that "character is a phase of personality comprising especially the more enduring traits which are of ethical and social significance" while the "goodness of character is determined by the degree to which conduct is directed intentionally to the common good" (p. 175). Dr. Long concludes that behavior among children is highly specific, and that character training must build up good habits by supplying specific situations for their functioning. He thinks that "mis-directed intention rather than inadequate understanding is at the root of most misconduct" (p. 186). In short, character training in children must be concrete and practical—by attractive examples rather than by formal precepts.

It may be said that the papers read at the meeting present a noteworthy agreement concerning the chief principles of the mental hygiene of childhood in relation to education. All agree on the need for scientific approaches, tempered, however,

by an enlightened sympathy for the condition and point of view of the child. All agree on the necessity of concrete and practical procedures, adapted to the comprehension of the individual child, in whatever situation he may be. There is a common rejection by the authors of the fatalistic implications of any one sided point of view, and a common perception of the complexity of situation in which the mental hygienist works. And all urge the continuity of education and mental hygiene.

These papers then, constitute a kind of symposium of the subject abounding in useful summaries of recent literature, as well as reflective interpretations designed to promote the further advance of mental hygiene in its educational applications.

University of Cincinnati

CHARLES M. DISERENS

La Pensée préconsciente. By C. KONCZEWSKI. Paris, Félix Alcan, 1939. Pp. xxi, 273.

This book, provided with an introduction by Pierre Janet, a foreword and preface by the author, but true to French type, with no index, is an attempt to prove for the "manyeth" time the rôle of affectivity in reasoning and all creative work. In a sense it is a metapsychology, in that it builds up a superstructure comprising man and the world, on the strength of analysis, largely introspective and of the armchair variety.

Janet, in his introduction, makes some pertinent critical remarks, telling us that the Polish author has exaggerated the neglect of affective psychology, and that these reflections on all sorts of great questions are not always very clearcut, nor very original, and that it is to be regretted that the author has scarcely given a historical outline of the matter. Apparently the author is a philosopher who likes psychological problems; and Janet's hope that he would some day, after continuing his studies, present us with a somewhat more precise research on a more definite subject is quite apropos and receives our approbation.

What strikes us at once as we dip into the volume is that much of this we had read in our youth. No, this is not a *déjà-vu* phenomenon. It can be accounted for the fact that M. Konczewski has apparently thought along the lines of his studies at the Collège de France and the Sorbonne. The book is dedicated to the memory of H. Delacroix, as "homage of my profound veneration." There are references to French philosophers and psychologists. In a tome on preconscious thinking, and assertedly dynamic psychology, Freud is mentioned parenthetically once! No German, British, or American psychologist is found in these pages, except for William James. (There is a discussion of substantive and transitive states, which should have been credited to William James.) Bühler is referred to, but his *Aufgabe*, in connection with imageless thought, which figures in the volume, appears as *tâche*. It is doubtful whether the author has read anything in German.

In short, it would seem that the book is a very much expanded seminary paper based on the views of the author's professors and their colleagues. The fact that much in the essay reminds us of James's writings and of the American pragmatists of 25 years ago should not surprise us in view of the kinship between James and French thought at the turn of the century.

The author goes to great length to demonstrate that most of our reasoning is grounded in subconscious elements, that there is a conflict between the environment and the "deeper self," a constant opposition of the external reality to the internal reality. Sometimes the one dominates; at other times the other. There is, therefore,

a duality of consciousness and also a duality of time; an inner time and an outer time, and the past is important in shaping the future.

In trying to find a striking idea to quote, we are confronted with what could have thirty or forty years ago been regarded as significant but now must be considered as either boringly commonplace or dangerously speculative, if not exploded. Let us take a few instances:

"The direct sentiment of our self is founded on that which one experiences at the time (*actuellement*). Every desire, every intense sensation is the affirmation of a self which manifests itself and which realizes itself through the mediation of the sentiments." (p. 99, italics his).

At the end of the chapter on "Preconscious Thought," the author formulates the following conclusions: "Preconscious processes directed by a sentiment of harmony are often very complicated and subtle operations. They cannot be regarded as inferior to conscious processes. . . . It may readily be seen that conscious judgments constitute most frequently the smallest part of mental work. They are very inexact and for the most part commonplace attempts at interpretation of a much vaster subconscious activity."

The most that can be said about the volume is that there are a number of stimulating passages, interesting quotations, that the filigree for the somewhat outmoded propositions, or else almost truisms in our present generation, is elaborately developed. As psychology, it is good impressionism, which leads us to the final bit of criticism. The author writes as if he had never heard of types or differential psychology. It seems as if all the various processes which he unfolds are supposed to function in the same way with every one, but perhaps he is thinking of facts that are superindividual, superpersonal and, therefore, superempirical—a sort of phenomenology in Husserl's sense.

Cambridge, Mass.

A. A. ROBACK

The Psychology of Physics. By BLAMEY STEVENS. Manchester, England, Sherratt & Hughes, 1939. Pp. xvi, 282.

The psychologist who turns to this book expecting to find a discussion of how the physicist works and thinks will be greatly disappointed. In fact, he will probably wonder why the word *psychology* appears in the title. The book is a discussion of theoretical physics, not of physical thinking.

According to Stevens, the fundamental postulates and concepts of physics are 'subjective' and not 'objective.' His 'psychology' of physics consists in stating these postulates and working out a theoretical system from them. Any concept which is in logical agreement with the postulates is called a *percept*. If Stevens had proceeded from such statements to an examination of how our perceived world of spatial and temporal objects governs our thinking about the abstract space and time of physics, we might accept Stevens' title. This, however, is never done. To be sure, he has stated in his conclusions that he has *shown* "that the laws" of physics depend upon the psychological organization which has been evolved in man's ancestors by natural selection" (p. 266). The demonstration consists, however, in the flat assertion that the basic postulates are subjective.

The significance of the word 'subjective,' an ambiguous term at best, is more confused than usual in Stevens' writing. In one meaning of the term one might argue that anyone may make his own physical theory of space and time and that

one theory is as good as another. Such an interpretation might be suggested by Stevens' criterion of the objective existence of the sun. The sun exists because we all see it, we also feel it, and we note its effect on climate (p. 5). This would seem to imply that subjective things are things upon which we do not agree. Stevens does not go this far, however. The postulates must be formulated in such a way that inferences derived from them will agree with the findings of modern experimental physics.

An alternative interpretation might be that these 'subjective' postulates are dependent upon the common features of man's thinking, and that they would be granted as obvious by all men. Stevens insists, however, that his postulates are not axiomatic and that they can be derived only by a process of search and testing.

Even if we grant that the statements made in this book about the subjective character of temporal and spatial postulates are meaningful, there is still a question whether a work which deals with the 'subjective' is necessarily psychological. If it were, all scientific writings would be psychology, since frames of reference of one kind or another must be introduced. The admission that one is using a system of classification or a coördinate system which is arbitrarily chosen for convenience in handling the facts of a science is scarcely a psychological description of the scientist's performance as he goes about his task.

Whether the book makes any important contributions to theoretical physics must be left for the physicist to decide. For those who may be interested in the work from the physical side it should be noted that Stevens presents an extensive theoretical system in a simplified form for students. By means of his system of definitions and 'perceptual' reasoning he claims that "one, after another the main axiomatic props of modern physics are shown to be fallacious; namely, the mass-velocity law, the Doppler effect theory, special relativity, . . ." (p. vii) and so on, for a list of seven more laws and concepts.

It is unfortunate that Stevens has decided to use the terms *psychology* and *perception* in his discussion. Aside from disappointing the psychologist interested in thinking, it may bring about more misunderstanding than ever about the nature of psychology among scientists in other fields.

Cornell University

T. A. RYAN

Psychologie und Psychiatrie der Conversion. By ERNST HARMS. Leiden, A. W. Sijthoff, 1939. Pp. 120.

In this brief work dedicated to C. G. Jung, Dr. Harms makes a substantial contribution to the rather meager literature on the psychology and psychiatry of conversion, regarded in a wider sense than in the classic work of De Sanctis. As Dr. Joh. Van Der Spek remarks in a special preface, the great significance of the treatise lies in its emphasis on social psychiatry in the treatment of disturbances produced by any fundamental change in the personal viewpoint of the individual. Dr. Harms proceeds very systematically discussing in succession the psychology, the psycho-pathology and the psycho-therapeutic treatment of conversion, with a final chapter presenting the attitude and guiding principles of a scientific social psychiatry.

Unlike De Sanctis, Dr. Harms does not limit the concept of conversion to that of religious conversion. A conversion, for him, is any transition or change of an individual from one world view to another. It may be philosophical, scientific,

economic, social, or political as well as religious provided the change produces a new orientation of the psyche, a new distribution and balance of personal energies, a new play of forces in the reciprocal actions of the person and his environment. In some cases this may amount to a disruption of the entire personality since the individual in question is faced by a persistent conflict between old habits, tendencies, beliefs, and social bonds, and the wholly different system of activities imposed by his new attitude. Dr. Harms, following Robert Sommer and C. G. Jung, conceives of the individual as a relatively closed system of psychical energies, a psychic monad, in relation, however, with other psychic monads and systems of monads, in the form of society and super individual systems of concepts, and moral tendencies. The point of view has much in common with that of Wm. McDougall. It is evident, therefore, that in cases of severe conflict due to a change in the individual's *Weltanschauung* of whatever kind, the effort of individual psychiatry may fail unless supplemented by a profound and sympathetic study of the successive and competing social environments of the individual subjected to the strain of conversion.

From the standpoint of individual psychology conversion is an exchange and transformation of the psychic contents of a personality in which thought, feeling, and will, may all be involved. It commonly begins in the unconscious and culminates in more or less conscious conflict. In the conversion process, which may or may not involve sublimation, the transformation of will and affect proceeds, most slowly and with the greatest difficulty and danger to the personality, a point of particular importance in the psychiatric treatment of such cases. Conversions of a reflective character, *i.e.* which shift the center of gravity of the personality from feeling to thought are safest from the psychiatric standpoint. Indeed such conversions may even stabilize the personality and improve mental health.

Conversion is, from the standpoint of the author, a phenomenon of social psychology, characterized by the conflict of the individual with, the super individual religious, political or philosophical setting in which he has grown up, which so to speak has formed his personality.

This is the pathogenic situation, and it is at this point that the psychopathology of conversion appears in many cases, and Dr. Harms finds that these are classifiable as schizoid or even schizophrenic, and paranoid types, which conform in appearance to the types recognized by individual psychiatry, but can only be improved or cured by an exhaustive study of social causation and a judicious adjustment of the social environment of the patient. A number of interesting pages are devoted to the depressions, phobias, paranoid phenomena, amnesias, and demented states of victims of psycho-pathological conversion. He even suggests that the perennially interesting case of Nietzsche may be an example of the type.

In the third section of his work, Dr. Harms offers a detailed study of eight patients suffering from psycho-pathological disturbances due to some type of conversion, and describes the methods of social psychotherapy actually employed with good results in all cases. The cure depends upon insight into the individual case, through a complete knowledge of his social relationships followed by the special social therapy of adjustment. We may call this process psycho-synthesis, or better reconstructive therapy, which aims at the reintegration of the individual with the social and mental world in which he is obliged to live. This must be pursued in the light of social characterology, *i.e.* a theory of types of social behavior, such as Willy-Hellpach proposes.

In conclusion, Dr. Harms urges psychiatrists to view their problems in the light

of philosophy and sociology as well as of psychology and medicine, and to develop to a kind of medical religion, a "Charitas-Religion," with no dogma save that of being beyond dogmas. His book may be warmly recommended to psychologists and psychiatrists not only for many interesting details and case studies but for its methodological value, and useful suggestions as to further studies in a field which has been comparatively neglected.

University of Cincinnati

CHARLES M. DISERENS

The Psychology of Common Sense: A Diagnosis of Modern Philistinism. By A. A. ROBACK. Cambridge, Mass., Sci-Art Publishers, 1939. Pp. 350.

At a first glance Dr. Roback's book seems to lack unity, although the titles of the fourteen essays, half of which have appeared in magazine form, are intriguing to the psychologist as well as the general reader. As one reads, however, a single theme will be found running through all of them, namely the *Leitmotif* of reason. The author only claims this for most of his essays, but even such light essays as those on "Marriage Lore" and "Music and Moronity" are eminently reasonable, even if somewhat temperamental. Particularly good are the essays on the definition of sanity, sex in dynamic psychology, the neurotic through the ages which presents an excellent brief history of the subject, the quack in psychology, the psychology of success, and the concept of significance.

Dr. Roback remarks that there are few or no books on the psychology of common sense, and that few psychologists, since the time of the Scotch school have made any appeal to it. To remedy this defect and illustrate the vagaries produced by the absence of common sense, he makes a brave attempt to achieve an adequate definition, by calling upon all the resources of etymology, logic, law, social science, and several other disciplines. In a sense he approaches his problem indirectly, by examining the meaning of a number of what are supposed to be related terms. In the last analysis common sense turns out to be good sense, sanity of a superior order, the ultimately rational—for much that passes as reasonable in any particular age is later seen to be absurd. There is, however, a kind of reasonableness which is perennial. If not immediately grasped as reasonable, it will be adjudged so in the course of ages. There is a fundamental significance to which thinkers in various ages are striving, which unfolds itself gradually as the result of human experience and which clear minded intellects not necessarily technical philosophers can intuitively perceive. Possibly this is what is to be understood by Spinoza's concept of *Scientia "intuitiva"* (p. 318). Here is the fundamental passage of the book.

Common sense is just this capacity to grasp what will finally prove perennially reasonable. As such it is very rare, indeed much more so than intelligence which can exist in the absence of common sense or sanity. As an authority on *Characterology*, Dr. Roback, contents himself, with a simple division of the intelligent into the sound and the surd, the latter a vast majority. Good tests of common sense, the capacity of the sound and thoroughly sane, would apparently be of far more importance than intelligence tests, and their future appearance is confidentially predicted.

Dr. Roback sometimes seems paradoxical in some of his statements, while others may be unwelcome to close adherents of particular schools, or to those who emphasize "newness" in psychology and entertain contempt for the psychology of the past. Actually psychology represents a continuous growth, and differences in terminology

or predilection for particular methods do not constitute any important novelty. "There is only one science of psychology although there are many methods, theories, and schools. The trunk of the great tree had taken root thousands of years ago. The 'new' psychologies of today are only branches that bid fair to conceal from view the stout trunk that was already in existence at the time of Aristotle." Although our laboratories have furthered our knowledge tenfold, "there is still a good deal in the 'Anthropologie' of Kant, in the 'common sense' observations of the Scotch school, not to mention the monumental textbooks of James, Wundt, and Ebbinghaus which will bear examination today; and furthermore, there is much in the so-called new psychologies of today which will with justice be forgotten in a few decades" (pp. 189 f.). Some contemporary psychologists at least will recognize that this is an eminently reasonable statement meriting frequent repetition.

Other points deserving attention in these essays are the recognition of Freud as the real founder of dynamic psychology, the protest against the contemporary child cult with its tendency toward the infantilization of adults, the frank declaration that much modern music—and the author might have added modern art in general—is significant of a low grade of intelligence or rationality, and an interesting discussion of freedom and the psychology of dictators. The author also criticizes popular conceptions of success, unfortunately promoted by certain psychologists. All such popular views and vagaries are typical products of Philistinism, or rather of the debased and short-sighted pragmatism which is the basis of the Philistine's attitude. Dr. Roback's essays are full of wit and wisdom and may even attract and convert some of the Philistines, whose follies afford him a background for his psychological picture of common sense.

University of Cincinnati

CHARLES M. DISERENS

Child Psychology. By MARGARET WOOSTER CURTI. Second edition. New York, Longmans, Green & Co., 1938. Pp. vii, 458.

In the opinion of the reviewer this edition of Dr. Curti's book is a decided improvement over the first. It is written in an interesting style, and is stimulating to the advanced and scholarly student. The author made no attempt to survey the literature in her field but she did report a sufficient number of the more important studies to cover the major areas and these were accompanied by pertinent suggestions for further research. The second chapter, in which one child is followed through her first five years of development is of considerable value, especially when elaborated in the class room by a general discussion of the preschool child's development. The discussion of hereditary and environmental factors in prenatal development and of the nature of the behavior of the prematurely born and of the neonate is tedious and, because of its highly technical nature, is of little interest to students taking their second course in psychology.

The sections of the book which especially appealed to the reviewer are those on the conditioned response, preceptual motor-learning of complex habits, reasoning, problems in mental life, social and ethical behavior, and the discussion of the integration of personality. The author's discussion of the origin of meanings, their growth, and their organization in reasoning is good, though too long and too involved for undergraduates. The section on reasoning centers largely around the work of Piaget.

The most serious defects of the book are as follows. The author presents an unanalytical and biased discussion of the effect of training upon pitch discrimination (p. 150 ff.), in which the ability to discriminate pitch is confused with the ability to reproduce various pitches audibly. In the discussion of the doctrine of inherited intelligence, she has accepted the results of Newman, Freeman, and Holzinger's study on twins without taking cognizance of the criticisms that had been made of their work. Furthermore, she has accepted at face value the Iowa studies on environmental effects upon the *IQ*. A section on the interests, play activities, and motivation of the child would have increased the usefulness of the book. A table of contents, rather than a mere listing of chapter headings is called for in a textbook.

Pennsylvania State College

EDWARD B. VAN ORMER

The Individual and His Society: The Psychodynamics of Primitive Organization. By ABRAM KARDINER. With a foreword and two ethnological reports by Ralph Linton. New York, Columbia University Press, 1939. Pp. xxvi, 503.

This volume represents a significant advance in the psychology of personality and belongs on the 'must' list of every social psychologist. In the tradition of Malinowski it examines Freudian theory against the background of anthropological evidence, but it goes beyond both Freud and Malinowski in integrating the functional societal emphasis of the ethnologist and the psychobiology of the analyst. The author develops constructively the implications of the Freudian neglect of the institutional pattern of a culture.

The thesis is that instead of a basic structure of personality, universal to all cultures, the ego is the product of the individual's reactions to specific institutions. This view is not novel but its elaboration and implementation by Kardiner are noteworthy. To the reader who has been impressed by the static nature of most dynamic theories of personality the four chapters comprising Part II will be the most valuable part of the book. Here, after Linton's ethnological reports of the Marquesan and Tanala societies, Kardiner works out a genuinely dynamic account of personality development. The interaction of specific institutional pressures, especially familial, and the needs of the individual are well described in terms of the anxieties and frustrations created in the personality, the defenses mobilized and the accompanying psychic elaborations. In place of the orthodox Oedipus complex appears an informative portrayal of the child's dependency upon adults as it relates to the particular disciplines and training of the culture.

The rest of the book is devoted to methodological and theoretical considerations. An excellent critical review of the social psychology of Freud is included. Not all psychologists will be completely satisfied with Kardiner's methodology which centers about genetic interpretation similar to that of the analyst. While Kardiner may not, however, make us all into neo-Freudians, he has made his psychiatric approach comprehensible, useful, and sensible.

From a systematic point of view objection may be raised to Kardiner's dichotomy which contrasts fixed, objective social institutions with the changeable, subjective patterns of personality. Social institutions are after all only patterns of human attitude and habit. In justice to the author, however, it might be added that he is aware of the problem. Moreover, even if all the unsolved problems of social psychology

are not answered in this book there is ample reason for satisfaction in its rich suggestiveness, its penetrating insight, its wise interpretation and its theoretical contributions.

Princeton University

DANIEL KATZ

Tides in the Affairs of Men: An Approach to the Appraisal of Economic Change. By EDGAR LAWRENCE SMITH. New York, Macmillan Co., 1939. Pp. x, 178.

This book is of interest to the psychologist primarily as an addition to that relatively small body of literature of which the studies by Huntington and Dexter are probably the most familiar. The author undertakes to show a relationship between weather and general business conditions. The latter he measures chiefly through the Index of Stock Market Prices, although certain other corroborative economic data are also included. The term weather is used to mean not the day by day changes, but the broader variations which might be described as "kind of weather" and are illustrated by such expressions as "a cold winter" or "a dry summer." A large part of the book is devoted to statistical evidence for a recurrent decennial pattern in stock market fluctuations, as well as to the comparison of annual changes in stock market prices which the concomitant variations in rainfall, temperature, and barometric pressure. At the same time, the author quotes data from astronomical writings which suggest a 3- to 4-yr. cycle in solar radiation, of which the changes in sun spots are a partial indication. The author points out that a 10-yr. cycle would result from the combination of a 12-mo. and a 40-mo. cycle, both of which are correlated with solar conditions. The underlying thesis of the book is that changes in the amount and quality of solar radiation may have a direct effect upon man's physiological and psychological condition which would in turn be reflected in the confidence, optimism, and other attitudes manifested in his economic behavior. Thus both the weather conditions and the economic changes would be direct results of the variations in available solar energy.

The most obvious criticism that can be leveled against the author's approach is the familiar one that causation cannot be inferred from correlation. None of the evidence presented shows more than concomitance. The aspect of human behavior under consideration, furthermore, is so complex and subject to so many influences, as to make it very unlikely that all other factors could have been adequately recognized or ruled out in the statistical comparisons which were made. The data presented fit the hypothesis only approximately and the generalizations are at best tentative, as the author himself points out. The author places much emphasis upon the fact that his predictions for 1937 stock market prices were subsequently verified and two chapters are devoted to these analyses. The predictions for 1938 and 1939, however, did not check so closely with subsequent developments and are dismissed in two pages. The book contains two chapters entitled "Tides in Mass Psychology" and "Speculative Psychology," respectively. The only psychology to be found in these chapters is in the titles. It would have been better for the general feeling tone and prestige of the book if the material in these chapters had either been omitted or condensed into one or two paragraphs and presented as the author's personal opinion rather than in the name of psychology. Regarded as psychology, many of the statements in these chapters are naïve or absurd, but it is perhaps fairer to the author to ignore the fact that he labels this sort of thing psychology.

Queens College

ANNE ANASTASI

Predicting Success or Failure in Marriage. By ERNEST W. BURGESS and LEONARD S. COTTRELL, JR. New York, Prentice-Hall, 1939. Pp. x, 472.

This book, based on 526 returns to a questionnaire concerning marriage, is essentially a research report but it is written in a style which should make it appeal to a fairly wide audience. The essential facts regarding the marriages and the social backgrounds of the participants were supplied, anonymously by 153 husbands, 317 wives, 30 husbands and wives together, and 15 husbands or wives with the assistance of an interviewer. The group studied represents a sample from 7000 couples to whom the questionnaires were distributed. For the most part, they are urban, middle class, Protestant, and native white. All were married from one to six years.

An index of marital adjustment based on 26 items in the questionnaire was used to divide the marriages into three criterion groups (good 43%, fair 28%, poor 29%). The response of each group to each of the other questions was then analyzed and the significance of differentiating responses evaluated by finding the critical ratio of the difference in proportion. On the basis of these differences, weights were assigned to various responses and a scale for predicting marital adjustment was constructed. For the couples on which the weights were derived, this prediction score correlates $+0.51$ with the adjustment score of the marriage. For a different sample of 155 cases, this validity coefficient was $+0.48$.

In line with their attempt to make the book readable, the authors have placed nearly half of their 114 tables in an appendix and all of the critical ratios in footnotes. Fifty-eight charts (bar diagrams) are used to portray the proportions of each criterion group making each response to the differentiating items. Five detailed case studies are also presented. The appendix contained a chapter bibliography of about 500 titles.

Methodologically, the research has flaws, most of which the authors point out themselves. One wonders if the findings would have been the same had the 6500 non-cooperating couples also returned their questionnaires. Appropriate analytic techniques were employed, but one wishes more attention had been given the patterning of husband-wife backgrounds and the relationship of these patterns to compatibility.

One cannot but make certain comparisons of this book with Terman's "Psychological Factors in Marital Happiness," published a year before. Both studies sought to find correlates of varying degrees of marital felicity and both resulted in "prediction scales" of marital compatibility. Terman studied the larger population, 792 marriages, and received responses from both husband and wife. Burgess and Cottrell studied 526 and in most instances secured responses from one spouse only. Terman's method of collecting his data probably resulted in franker responses and less collusion between spouses. Terman's data included information on personality, social background, and sexual adjustment where as Burgess and Cottrell's data deals almost exclusively with social background. Both studies, however, offer encouragement to those who believe that even complex social problems involving multiple causation can profitably be made the subject of research.

Purdue University

E. LOWELL KELLY

A Study of Retarded Children in the Elementary School. By WILLIAM MCGEEHEE. Nashville, George Peabody College for Teachers, 1939. Pp. 128.

This monograph presents the statistical findings of the investigation of retarded

children in 36 states, 310 communities, and 455 schools in the United States. The children were of both sexes, from fourth through eighth grades. For the purposes of comparison, four groups are recognized. Group one is normal, and is here considered purely for purposes of comparison with the other groups. The second group consists of the 4,627 children whose scores on Kuhlmann-Anderson tests comprise the lowest 10% of the distribution. The third group consists of 3,359 children whom the various teachers had classified as "retarded" before the above mentioned intelligence test had been administered. The fourth group is composed of the children who fall in both the second and third groups mentioned above, and they are called the "lowest 10% and retarded" group.

Chapters III to VII are concerned with comparisons of the mental status as revealed by the intelligence test and the following factors: (a) Scholastic achievement as revealed by certain standardized educational tests arbitrarily chosen. (b) The personalities of these children as revealed by scores on the B. P. C. Personal Inventory and teachers ratings on some 70 personality traits. (c) The interests and social adjustment as revealed by extra curricular activities, hobbies, and play interests. (d) The environment as revealed by the father's occupational status, certain social economic criteria, size of family and rank in birth order.

The work as a whole is top heavy with expansive tables, the significance of which would have been much more easily grasped in graphical representation. The body of the book is padded with obvious comments on the content of these tables. The ground which the study covers has been cultivated for many years, and nothing whatever new is discovered, but one of the conclusions might be restated here, even if it only corroborates well established truisms.

There is a preponderance of males in all the 'retarded' groups. Insignificant differences in intelligence as revealed by the test can not account for this. The explanation of this condition seems much more closely related to personality conditions, in which the boys show significant tendencies toward maladjustment. Since this distinction is based upon estimates of children by teachers, it seems likely that the traditional "school ma'am" is ruffled unduly by the more aggressive, truculent, and defiant young males and shows a decided preference for the more docile "nice little girls."

This monograph confirms a number of previous findings, and may serve as a valuable source book for investigators with more advanced statistical techniques who may wish to analyze more fully the 'retarded' children.

Iowa Child Welfare Research Station

N. J. VAN STEENBERG

Their Future Is Now. By E. M. LIGON. New York, MacMillan Co., 1939. Pp. iv, 369.

Their Future Is Now sets forth its author's system for correcting personality maladjustments and for child guidance, in terms of the teachings of Jesus Christ. Eight "personality goals" entitled, "Vision," "Love of righteousness and truth," "Faith in the friendliness of the universe," "Dominating purpose," "Being sensitive to the needs of others," "Forgiveness," "Magnanimity," and "Christian courage," form the basis of the system proposed. Each personality goal is traced through nine age-levels which cover the period from birth to maturity. In addition to a preface and historical introduction, there are 15 chapters which bear the following titles:

"The Nature of Character and Personality," "Dynamics of Character," "The Personal Equation," "Their Future Is Now," "Before the Nursery," "Character Development in the Nursery," "The Kindergarten Child," "The First School Years," "The Third and Fourth Grades," "Middle Childhood," "In Junior High School," "The High School and College Age," "The Mature Personality," "Social Integration" and "Happy are They."

One is impressed upon reading this volume with the intense sincerity of the author, and of his highness of purpose. The principle, often emphasized, of supplanting socially undesirable reactions with socially desirable ones is sound and workable. The style of writing is clear and direct. Yet the book contains entirely too many religious quotations and references to the Bible, and would be improved if most of them were omitted.

Despite the fact that the author has much to say about a scientific approach to the problem of personality, the present reviewer seriously questions the adequacy of the scientific attitude demonstrated. The ideal of personality measurement is excellent, but the means of measuring personality are confined in the main to the author's own rating scales and to his own questionnaires which are given at the ends of several of the chapters. We have been unable to find any reference to the reliability of the rating scales or questionnaires, beyond the suggestion that two or more raters are better than one. The ability of the raters to make valid ratings seems to be taken wholly for granted.

Statements like the following will be particularly objectionable to the scientific reader and make one wonder just how far the author pressed the scientific method in the evaluation of his own writing. On p. 4, for example, is the definition that, "Character consists of the characteristic forms of reaction of the personality." On p. 166 we are told that, "Sympathy is a natural, inherited attribute in human nature." We learn on p. 183 that, "During the years from eight to fourteen physical courage is the most desirable of all personality traits." And p. 185 informs us that the period of the third and fourth grades "is the first period at which true poverty of spirit can be developed." In making such statements it would appear that the author has let his religious fervor run away with his scientific judgment.

The group that *Their Future Is Now* is likely to interest most is the church laity, rather than technically trained psychologists.

Indiana University

W. N. KELLOGG

Sexual Pathology: A Study of Derangements of the Sexual Instinct. By MAGNUS HIRSCHFELD. Revised edition. New York, Emerson Books, 1940. Pp. 368.

The first part of this book consists of case records of fetishism and anti-fetishism collected by the late head of the Institute of Sexual Science of Berlin. It is not constructive, either from the point of view of theory or that of therapeutics, but forms an interesting and useful background for the rest of the book.

Abnormalities of the quantitative variations of the sexual urge are more scientifically discussed in the chapters on hypereroticism and its deviations, polyeroticism and superfixation. Much consideration is given to divergences from normal cohabitation, and to masochism and sadism. Sadism is "a sort of atavism"—a reversion to primitive violence more or less inherent in the nature of the sexual act, but "overcome normally through the development of restraining or sublimating mechanisms." The true "lust murder" is usually committed not by the oversexed, but by

the undersexed, in a frenzied rage at his own relative impotence and the resistance of his victim. The treatment of hypererotism should be "symptomatic and casual, with medicinal and psychic sedatives" (suggestion). Dr. Hirschfeld never advised castration, but found it beneficial when voluntarily undergone.

Part III of the book is devoted to a thorough discussion of the causes, manifestations treatment of impotence. There may be cerebral impotence, with lack of urge or pleasure; spinal impotence, with disturbances to erection or ejaculation; genital impotence, with organic defects which prevent cohabitation; and germinal impotence or sterility. All of these may be accompanied by psycho-neurotic strain. There should be a trial period of a year or two before impotence is judged chronic and incurable. Treatment should be based on exact diagnosis. Organically conditioned disorders should combine general treatment with that of the basic ailment, but local measures forcing anxious attention on the region may increase psychogenic factors and should not be overemphasized. In spinal disorders, strychnine, galvanization of the vertebral column, and Roentgen rays, are used. Functional disorders must have *active* psychotherapeutic treatment of the whole neurotic condition. The author has "never seen final cures result from psychoanalysis alone," though it is useful in revealing restraining psychic factors. Suggestion during hypnosis and also masked behind physical methods such as stimulating medicines, is curative. *All sexual intercourse* was forbidden during treatment. He regards the use of glandular extracts as less effective than expected, but hopes their use may be perfected.

University of Michigan

BERENICE BARNES SHEPARD

Correcting Nervous Speech Disorders. By MABEL FARRINGTON GIFFORD. New York, Prentice-Hall, 1939. Pp. 197.

Within the past few years there has been a growing interest on the part of physicians, psychologists, educationists, endocrinologists, and speech pathologists in the subject of stuttering. The explanations of the etiology of stuttering are at least 16 in number, and the recommended therapeutics are even more numerous. Speech correctionists of various schools of belief concerning stuttering claim that their methods beget results. There is a growing belief among speech specialists that stuttering and allied speech disorders may be caused by a variety of conditions—physical, psychological, and environmental—and that a variety of therapeutic techniques are needed to correct the speech of various stutterers.

Since all workers in the field of speech correction are in need of references and descriptions of various techniques now being used in the field, it is a real help to have a description of the therapeutic procedures used by the Bureau for the Correction of Speech Defects and Disorders of the California Department of Education. "Correcting Nervous Speech Disorders" is the result of Mrs. Gifford's work as director of the California program.

Mrs. Gifford states that the purpose of her book "is to bring into focus the technique for the correction of stammering that is practiced in the California public schools." She has written the book primarily for speech therapists, teachers, and administrators who are interested in what is being accomplished in California with regard to remedial procedures. The principles of Mrs. Gifford's technique are based on the premise that stuttering has no organic or functional origin, but is a problem of emotion maladjustment involving the total personality.

The book is conveniently divided into six parts: I. The Teacher and the Field;

II. The Principles of the Remedial Technique; III. The Remedial Technique; IV. Home Cooperation; V. Psychological Readjustment and VI an appendix of thirty pages dealing with record forms, case studies, etc. Mrs. Gifford's psychological principles are eclectic. She selects from various schools of belief those psychological principles that she has found helpful. Conditioning, mental hygiene, suggestion all find a place in Mrs. Gifford's corrective program. Psychologists interested in a way of treating stutterers, stammerers, and clutterers will find this book helpful. It should also find a place in the library of every speech correctionist.

Queens College

JAMES F. BENDER

A Textbook of Clinical Neurology. By ISRAEL S. WECHSLER. Fourth edition. Philadelphia, W. B. Saunders Co., 1939. Pp. 844.

A knowledge of clinical neurology is a very useful adjunct to the teaching of psychology. It is often hard to motivate students to study the structures and functions of the nervous system, because these topics seem dry and academic. The normal functions of the cerebellum, for example, may fail to elicit the understanding and enthusiasm of college students, but the effects of a cerebellar tumor are quite striking, and a brief description of this abnormality may turn a dull topic into an interesting one. Even in introductory classes, a judicious use of a little clinical neurology may be profitable.

Wechsler's textbook is a new revision of a standard work, the preceding edition of which appeared in 1935. The greater part of the book (577 pages) is devoted to exhaustive descriptions of the disorders associated with organic pathology of the nervous system. The approach is distinctly clinical, with the emphasis on the symptoms, diagnosis, and treatment of each disorder. The writing is competently done, and, except for medical terms that will cause most psychologists to reach for a medical dictionary, the book is not unduly difficult to read. The index is very serviceable. The bibliographic references are not extensive and few of them are very recent, but this will not detract from the use of the book by psychologists.

Psychology is directly represented in the book at two points. A brief section (15 pages) on psychometric tests is contributed by David Wechsler. Even with allowance for the limitations of space, this section has serious shortcomings. A number of concepts, as, for example, that of the average adult mental age, are treated with conspicuous lack of precision. At one point it is recommended that psychometric examinations be given only by qualified psychologists, yet at the end of the chapter instructions are given for administering a brief battery of tests. It is doubtful that these "correlate very highly with general intelligence as measured by the Binet-Simon tests," as is asserted.

A second psychological topic is found in an extended section on the psychoneuroses (64 pages). These are considered entirely from a psychoanalytical point of view. The only mention of any objective or experimental approach to the psychoneuroses consists of a few disparaging remarks. Psychologists may well conclude from this section that the kind of psychology known to neurologists is an inadequate as the brand of neurology known to the average psychologist! The psychological shortcomings of the book do not detract, however, from its value as a compendium of reference material on the organic disorders of the nervous system.

Carnegie Institute of Technology

LORANCE F. SHAFFER

The Psychology of Parent-Child Relationships. By PERCIVAL M. SYMONDS. New York, D. Appleton-Century Co., 1939. Pp. xiv, 228.

This book is an attempt to study the rôle of emotional security within the home in the development of child personality. The greater part of the volume is concerned with a statistical compilation and discussion of findings from case studies representing extremes of two psychological continua regarded by the author as basic aspects of family relationship. The first of these, the acceptance-rejection continuum, is here represented by 31 matched pairs of Ss selected by former students of the writer, who also prepared the case studies under his direction. One member of each pair was regarded as a rather extreme instance of parental rejection, the other as an equally marked example of parental acceptance. The two members of each pair were matched roughly as to age, school grade, sex, intelligence and social status. The complete age-range of the group was from 5 to 23 yrs.

The second aspect of family relationships studied was dominance or submission on the part of the parents. Again the method of matched pairs was employed. Twenty-eight pairs of Ss with an age range from 6 to 17 yrs. provided the data. In each of the two studies, the characteristics of the Ss in the contrasted groups are subjected to statistical comparison. Evidence for the better social and emotional adjustment of the "accepted" children when compared to the "rejected" cases in the first study seems very clear-cut. In the second study, fairly marked differences in the direction of greater aggressiveness on the part of children of "submissive" parents with greater docility shown by those whose parents were "dominating" appeared.

Remaining chapters in the book are devoted to reviews of the literature on parent-child relationships, teacher-pupil relationships, and councillor-client relationships. Although the author makes no claim to having solved the riddle of interpersonal relationships within the home and school, his discussions of the complicated interlinkages of these factors with the developing personalities of children will be read with interest by clinical workers and students of social psychology.

University of Minnesota

FLORENCE L. GOODENOUGH

La conduite humaine et les valeurs idéales. By D. PARODI. Paris, Félix Alcan, 1939. Pp. 138.

In the present volume, a sequel to *En quête d'une philosophie* (1935), the author analyzes the ideals in the classical triad: Truth, Beauty, Good. On the postulate of a "tendance universelle," or continuity of inner tension (Bergsonian "élan"), attempt is made to fuse Plato's "being and becoming," certain nineteenth century teleologies, and dynamic psychology. The whole is reduced to a system superimposed by a mind-will. Logical eclecticism on a strong transcendental bias pervades the piece.

Short shrift is made of pragmatism and psychological determinism as too delimiting for the cadre in which the philosopher seeks to operate. Possibilities in an empirical approach are suggested only to be discarded, less the contemplation of ideal values be reduced to an analysis of intellection, esthetic pleasure, and moral sentiment in their "éléments sensoriels." There is further concern, lest such an approach render religion, like metaphysics, "a vain mirage condemned to vanish before the progress of positive science."

Despite the author's brilliant use of dialectics and nuance (with which his native language is fecund) one detects a kind of noble psychoanalytic scheme wherein the method for determining ideals is as transcendental as the ideals themselves. At times M. Parodi narrowly risks getting inside his system and intellectualizing. In psychological conceptualism he leans toward dynamic psychology as more fertile to his method.

To decry the philosophical import of the work would be tantamount to mental parochialism, for the master logician is everywhere evident in the coherence of his structure. His most provocative insights lie in his historical treatment of the genesis, unification, and divergence in ideal values. On the literary side, the style is always clear. Occasional lyrical passages, which in less mature hands might conceivably slip into the evangelical, are saved by the dialectic severity the author has imposed on his task.

In short, if, instead of offering his philosophy as a handmaiden of the sciences, M. Parodi turns the tables—he must be assailed not as a philosopher, but as a psychologist.

University of Maryland

WALTER M. SPARKS

Beyond the Clinical Frontiers. By EDWARD A. STRECKER. New York, W. W. Norton & Co., 1940. Pp. 210.

Dr. Strecker is Professor of Psychiatry in the Undergraduate and Graduate Schools of Medicine, University of Pennsylvania, and Consultant and Chief-of-Service, Institute of the Pennsylvania Hospital. The book is one of the annual series of lectures of the Thomas W. Salmon Memorial Lectures of the New York Academy of Medicine. In a foreword to the lectures the writer says, "these lectures will discuss briefly a few of the crises which are menacing our civilization and its cultures. There will be some attempt to accent the psychiatric implications of these crises. And, finally, there will be sketched in, at least, the crude outlines of an alleviating plan of hygiene that psychiatry might be able to offer to a mentally sick world."

The chapters are a series of non-technical essays insofar as language goes and should prove to be interesting reading to a large group of thoughtful professional people outside the medical world: sociologists, psychologists, biologists, historians and all who must reckon with man's frailties in ratiocination, his mob-crowd escape mechanism, his weakness for propaganda, and his stubborn adherence to a world of shadow with all of its cruelty, social lag, and flight from reality.

The book is easy to read but far from easy reading and hardly a page but provides a challenge if one reads without trying to escape the inevitable logic involved in the implications. Pushed to its logical conclusion there is always the philosophically unthinkable blow to the ego; that is a thoroughgoing determinism which makes of volition and free will a shadowy concomitant of what is already a biological *fait accompli*. Even bio-hereditary matters may be but minor cogs in a more cosmic determinism. This Strecker ignores but skirts dangerously. Such an idea would require a super-escape mechanism and would curdle any "alleviating plan of hygiene" that (Dr. Strecker's) psychiatry might be able "to offer to a sick world."

The social-psychiatric diagnosis in which Strecker compares the emotionalized thinking of crowds (blocks, nations, races, creeds, pressure groups) to the behavior of unsagacious neurotics is quite convincing. His therapeutic suggestions of a psychiatric nature are not so convincing.

University of Pittsburgh

W. T. ROOT

Lecture Demonstrations for General Psychology. By NORMA V. SCHIEDEMANN. Chicago, University of Chicago Press, 1939. Pp. x, 241.

Dr. Scheidemann describes, in this little book, 60 demonstrations which she has adapted and condensed from experiments widely scattered in the psychological literature of the last 30 years. Due acknowledgment and complete citations are given to the source material, hence the interested reader may readily obtain further information concerning the facts and principles exhibited.

Besides the selection of the topics to be demonstrated, Dr. Scheidemann's contribution consists in presenting the materials in a convenient and usable form. The following outline is followed in every one of the 60 demonstrations: (1) the purpose is briefly and clearly stated; (2) the materials required are listed; (3) the procedure is fully described; (4) points of special interest to the class are enumerated; and (5) comments are made upon the original investigator's work.

The demonstrations require no special apparatus nor equipment, beyond that readily available to all teachers, and they may easily and quickly be made. They cover a broad field: sensation, reaction, perception, attention, memory, learning, recognition, recall, suggestion, thinking, belief, testimony, personality traits, etc. Teachers of general, educational, experimental, and applied psychology will find material suitable for their use.

The pertinence and significance of the demonstrations vary widely. Some are first rate, others are trifling and of doubtful value. Despite the obvious unevenness and inequalities, Dr. Scheidemann's book supplies a long and widely felt want. It is particularly useful to teachers lacking the facilities of a well equipped psychological laboratory.

K. M. D.

The Dream World: A Survey of the History and Mystery of Dreams. By R. L. MÉGROZ. New York, E. P. Dutton & Co., 1939. Pp. xvi, 319.

The author brings together in most readable form the beliefs and practices which have been associated with the marginal consciousness of dream experience. The book covers a wide range, from the early historical period to the present and from many cultures both advanced and primitive. There is a wealth of illustrative material; an anthology of dreams, it might be called. Preference in selection is naturally given to dreams that are extraordinary in themselves or are associated with persons of note.

Apprehension of the unknown past and divination of the unknown future, clairvoyance of the present, and telepathy, all seem credible as one reads these narratives. Yet the author makes no dogmatic evaluation of past or present theories, but concludes "that the serious consideration of certain aspects of dreaming beyond the scope of psychoanalysis must compel philosophy to take new bearings and dissipate the new materialism that befalls a Freudian machine-age."

Himself a literary critic, the author discusses "creative dreaming" and its influence on the work of men like William Blake and others. Of Christina Rossetti he says, "Few poets have been so haunted by dreams and unfulfilled longings."

The psychologist will find this book a valuable source of illustrative material. The "general reader" will find it as interesting as his favorite mystery story magazine and better written at that.

Ohio State University

FRANCIS N. MAXFIELD

Toward Proficient Reading. By JAMES ALEXANDER HAMILTON. Claremont, Calif., Saunders Press, 1939. ix, 152, 4 pl.

This concise little book contains a wealth of practical information for the reader who wishes to improve his reading rate and the comprehension of what he reads, and for the teacher who must deal constructively with reading problems. It has short but adequate chapters on the visual mechanism, analysis and improvement of eye movements, developing comprehension and speed, vocabulary development and the hygiene of vision. Case histories reveal the effectiveness of the methods employed by the writer in his clinical treatment. An appendix of 35 pages includes a vocalization survey, saccadic exercises, and progress reports.

There are any number of explicit directions for self-analysis of difficulties and numerous exercises and tests which require little apparatus and no more than average intelligence to try out on one's self. For the busy teacher, it is an excellent book to place in the hands of students who complain of poor skill in reading.

Dr. Hamilton proves himself a good psychologist in writing this volume. He recognizes that one essential of a guide book for poor readers is brevity; otherwise the material would be less likely to reach the audience for which it is intended. The reviewer also likes the modesty of the title. It does not promise a sure cure for all reading ills. It merely suggests that one may advance in the direction of the goal by applying the information found within its covers.

Western Reserve University

CALVIN S. HALL

An Introduction to Experimental Psychology. By C. W. VALENTINE. 3rd ed. London, University Tutorial Press, 1939. Pp. x, 283.

This is the third edition of a manual which first appeared in 1914. It is intended for students of education who have had no previous training in psychology. Of the 53 exercises included, 18 deal with learning and mnemonic processes, 8 with imagery and association of ideas, 7 with perceptual processes, 6 with motor dexterity, 4 with attention, 3 with intelligence, 3 with thought processes, 2 with mental work and fatigue, 1 with concept formation and 1 with correlation. All necessary materials are included. Part I gives the experimental procedures and materials while Part II discusses the results to be expected from the exercises performed.

Of the 166 appended references only 26 are from American sources. Nevertheless, instructors called upon to teach an elementary course in experimental educational psychology will find this manual quite useful, either as a text or for reference purposes. The manual contains much more material than any other of its kind that has come to the reviewer's attention. Although the comments in Part II are brief, they serve as valuable orientation for the student who may wonder concerning the relation of the various exercises to the problems of teaching.

Vanderbilt University

NORMAN L. MUNN

A Psychology of Music: The Influence of Music on Behavior. By CHARLES M. DISERENS and HARRY FINE. Cincinnati, Ohio, College of Music, 1939. Pp. 405.

The subtitle of this interesting book is more appropriate to the treatment than the main title. It is not a technical psychology of music but rather the practical musician's evaluation of theories of the origin of music, its rôle in mythology, folk-lore, magic, religion, medicine, psychiatry, and daily tasks. This comprises the first ten

chapters. The eleventh chapter, "Contemporary Experimental Work on the Psychology of Music," is comparatively superficial, groping for every floating straw that may have practical value and ignoring the type of work represented in scientific laboratories.

State University of Iowa

CARL E. SEASHORE

Eine tiefenpsychologische Grundlage zur Klages'schen Graphologie. By S. V. MARGADANT. Amsterdam, N. V. Noord-Hollandsche Uitgeversmaatschappij, 1939. Pp. 153.

The author is a Dutchman who writes from the Dutch East-Indies in the German language, because he discusses the theories of a German graphologist. He attempts to point out what is common in the psychological doctrines of Klages, Freud, Adler, and Jung, in so far as these doctrines can be applied to the field of graphology. Among the peculiarities of handwriting discussed the reader finds regularity, irregularities, height of script, speed, pressure, breadth, angle, method of tying, direction, simplicity, ornateness. Those psychologists who are interested in graphology in general and in that of Klages in particular, will find the book clearly written and easily readable. There is much more in it than mere graphology.

University of Miami

MAX F. MEYER

Indétermination et création. By LEONE VIVANTE. Translated by L. E. Lanza Paris, Fernand Sorlot, 1939. Pp. 270.

This book presents a theoretical discussion of universal determinism, with especial attention to the conflicting views of philosophical thought and the biological and physical sciences toward determinism. Universal determinism is defined as a principle of rare quality on which depends the sensible and intelligible world, and is likened to psychic truth. The book contains little of especial value to the psychologist.

Beaver College

ELINOR J. BARNES

Der Wille als Weg zu Leistung und Persönlichkeit. By ERICH GRASSL. Leipzig, J. A. Barth, 1939. Pp. 58.

A short pamphlet on "Will as a way to achievement and personality," based on a series of popular lectures. There is emphasis upon the genetic, the pathological and the educational aspects of the problem. For the scientific reader this booklet is of little interest.

University of California

EGON BRUNSWIK

Emotions and Bodily Changes. By H. FLAENDERS DUNBAR. Second edition. New York, Columbia University Press, 1938. Pp. xi, 601.

This book, representing "a survey of the literature on psychosomatic interrelationships," is not a revision of the original publication, which appeared in 1935 but merely "a second edition with supplementary introduction and additional bibliography."

The approach to "the problem of the psyche-soma" is largely clinical. The case studies and specific syndromes are highly informative and illuminating. As a text or reference book it is of interest to the psychiatrist and the medical man. From the standpoint of the psychologist it is less satisfactory. As in the first edition,

the extensive research and quantitative treatment of emotional processes undertaken by psychologists is given little or no attention.

In his introduction to the second edition the author justifies this omission on the ground that the psychologist interested in measurement, has tended "to concentrate on the parts without reference to the whole," and that his findings, therefore, have "added nothing fundamental in point of view and to the purpose of this book." Just how the description of specific syndromes and of physiological changes arising under conditions of emotional disturbance give a clearer picture "of the whole" than do the measurement of these same processes under laboratory conditions is not explained.

The material of the text is organized under three main heads. In the first division attention is given to orientation and methodology. In the second, the various organs and organ-systems affected by emotions are described and changes induced under clinical conditions are noted. The third division is devoted chiefly to therapeutic considerations.

Temple University

FREDERICK H. LUND

My Mind a Kingdom. By GEORGE THOMAS. New York, E. P. Dutton & Co., 1938. Pp. 293.

This is the story of the everyday doings of a shut-in family living in a tenement district of London, four of whose members, including the author, are afflicted with progressive muscular atrophy. It is told in the form of a very readable, 365-day diary. It is mainly, however, the story of the author's struggles against insuperable difficulties in becoming a successful writer, and an exposition of his philosophy of life.

While the book is not a treatise on psychology, as the title might suggest, the student of the psychology of adjustment will find here a rare example of the integration of personality under circumstances that might easily overwhelm the average person. "Everything in life, or nearly everything, encourages me to lie down and die, but I shall not do so—even to please the *Times Literary Supplement*." He can say this because all the elements of life fit snugly into his basic philosophy of life, which in this case is the Christian philosophy in its most ancient form, "because death, not life, is the problem."

University of Cincinnati

L. A. PECHSTEIN

Psychiatric Clinics for Children. By HELEN LELAND WITMER. New York, The Commonwealth Fund, 1940. Pp. xix, 437.

In this account of psychiatric work with children, Dr. Witmer has admirably fulfilled a three-fold aim. First, she has given a clear account of the basic philosophy underlying the establishment of psychiatric clinics for children, together with a historical account of the social and cultural developments that pointed the need for some kind of organized program of preventive and remedial work. This is followed by a detailed account of the present status of clinical work for children in America, especially that conducted under state auspices. The final section of the book is devoted to an unusually straight-forward attempt to balance the accounts of the clinical work thus far accomplished with view to ascertaining which aspects of it have yielded sufficiently worth-while results to warrant their continuance and which have been relatively unproductive. On the basis of a careful study of the

available reports, together with her own very extensive acquaintance with clinics and clinicians throughout the country, Dr. Witmer comes to the conclusion that there is little factual basis for the claim that the establishment of psychiatric service for children has had or is likely to have any measurable effect upon the frequency of psychoses or the extent of juvenile crime. Greater emphasis upon the improvement of the present mental health of children and less preoccupation with future contingencies appears to be a more promising line of attack.

University of Minnesota

FLORENCE L. GOODENOUGH

Mythes et dieux des Germains. By GEORGES DUMÉZIL. Paris, Ernest Leroux, 1939. Pp. xvi, 157.

A scholarly study of the prehistoric system of Germanic religion. After an introduction dealing with the relationship between Indo-European and Germanic mythology the author presents and discusses the myths of "sovereignty," of "the warriors," and of "vitality" (the latter dealing with fertility, drinking, etc.). There is a great deal of reference to the writings of the Romans, especially Caesar. The concluding chapter emphasizes the militarism inherent in Germanic mythology, as embodied especially in the figure of Odhinn (Wotan), and points toward similarities with the present "magico-military" structure of society and of political attitude in Germany.

University of California

EGON BRUNSWIK

Statistics Applied to Education and Psychology. By CLARENCE T. GRAY and DAVID F. VOTAW. New York, The Ronald Press Co., 1939. Pp. xiv, 278.

In this book the derivation of formulae is kept at a minimum, but extensive empirical evidence is presented as to validity for many of the formulae. The arguments are simply and clearly stated, and this, in conjunction with the many experimental demonstrations of why the formulae function, make it an excellent text for beginners in the subject. Numerous problems are presented at the end of each chapter, many of these sufficiently ingenious to make even the better students think.

This text treats the usual measures of central tendency, variability, sampling, and measures of relationship, but does not include such topics as analysis of variance, multiple regression, and factor analysis. In reading the book, one minor error, at least by implication, was noted on p. 169 where the authors state, "By mathematical definition, rectilinear correlation is the slope of the straight line which best fits the y values of the points in a correlation chart." This textbook, nevertheless, deserves careful examination by those teachers whose students lack an adequate training in mathematics for more advanced works but who desire a working and reading knowledge of elementary statistics.

University of Rochester

JACK W. DUNLAP

Out of the Running. By G. GERTRUDE HOOPES. Springfield, Ill., Charles C Thomas, 1939. Pp. xvii, 158.

Out of the Running is the intimate, very frank, highly speculative, well-written autobiography of a victim of intracranial birth lesions, who resolves the violent conflicts between her pronounced ambitions and love of life and her physical limitations by seeking refuge and solace in the authority of religion, which has brought peace and comfort to her turbulent life. Although the orthodox student of psychology will hardly agree with the author's decidedly psychoanalytical ex-

planation of her reactions, he will find here an excellent case study, since the author gives us a detailed picture of her overt behavior and most intimate reflections.
University of Cincinnati L. A. PECHSTEIN

The Story of Yiddish Literature. By A. A. ROBACK. New York, Yiddish Scientific Institute, 1940. Pp. 448, with 12 portraits.

The Yiddish language, which came into being when Jews settled on the banks of the Rhine (possibly even before Julius Caesar saw that river) and learned the speech of their German neighbors, is now the family idiom of most Jews. Although Yiddish uses the Hebrew alphabet, the two languages are very different. Hebrew, the language of the Bible, belongs to the Semitic group of languages, being akin to Arabic as well as to the now extinct Phoenician, Aramaic, Assyrian, and Babylonian; while Yiddish is a European language related to German, Dutch, English, and Scandinavian.

Lucid and informative, the *Story of Yiddish Literature* begins with the early medieval period of poetry, romances, *Purim*-plays, homilies, and medical treatises. The bulk of the book is devoted to the modern period from 1864, when Mendele wrote *Dos Klein Mentshle*, to 1940. Several chapters deal with Yiddish literature in America. Another chapter deals with scientific literature.

A product of 19th-century nationalism, modern Yiddish literature is sociologically significant in two other ways. It is the most international literature in the world. While English literature is domiciled in half a dozen countries, Yiddish literature is created and appreciated in nearly every country of the world, Old and New. This literature, moreover, thrives under unique competition. The competition comes from modern Hebrew and also from the language of the country. Thus the literate American Jew looking for a periodical or book of fiction may choose among English, Hebrew and Yiddish.

An important problem in social psychology is broached on p. 41 ff., where the factor of imitation in cultural borrowing is qualified by the principle of national or cultural selection. It may be added that the selective principle is also operative in the individual's appropriation of the culture of his group.

Two bibliographies and three indexes are usefully appended to the book. The first bibliography includes a list of works (poetry, short stories, novels, and plays) available in English translation. Most fully represented in the list are the novelists Asch and Singer and the playwright Pinski.

Cornell University

S. FELDMAN

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By F. NOWELL JONES, University of Alabama

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